

# ESSAYS ON THE ECONOMICS OF CRIME

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PhD Dissertation in Economics

presented by

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to

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degree of Doctor of Philosophy in Economics

## DECLARATION

I, Eva Marieke Schnabel confirm that:

- The work presented in this thesis is my own.
- Where information has been derived from other sources, I confirm that this has been indicated in the thesis.
- Chapters 1 and 2 contain jointly authored research work together with Professor Costas Meghir (Yale University) and Professor Mårten Palme (Stockholm University).
- Chapter 3 is sole authored research work of my own.

Chapters 1 and 2 are based on the working paper 2011:13 that appeared in the working paper series of the Department of Economics, Stockholm University, entitled "The Effect of Education Policy on Crime: An Intergenerational Perspective". The presented research in Chapters 1 and 2 are a major revision (including extension of data) of this initial working paper.

Joint contribution of the authors Costas Meghir, Mårten Palme and myself to Chapters 1 and 2:

- Design and development of research project
- Decisions about methods used in analysis
- Interpretation of data
- Writing of draft

My contributions to the joint research presented in Chapters 1 and 2:

- Completion of data cleaning, combination of various data sources, coding of variables, coding of Swedish law to match variables
- Preparation of various data sets for statistical analysis
- Perform data analysis
- Produce outputs for presentation of results, such as tables and graphs
- Documentation of data cleaning, coding, data preparation and data analysis

Sole contribution of Mårten Palme:

- Acquisition of data
- Reform assignment coding

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## Abstract

This thesis considers public policy effects on crime in Sweden using extensive administrative register data on all convictions in Sweden between 1973-2010. First, it explores the impacts of the Swedish compulsory schooling reform that took place between 1949-1962 on individual crime of the generation directly targeted by the education reform. Then it considers the intergenerational effect of this education policy on crime. Policies are often evaluated on either short term outcomes or just in terms of their effect on individuals directly targeted. If such policies shift outcomes across generations their benefits may be much larger than originally thought. This study provides novel evidence on the intergenerational impact of policy by showing that educational reform in Sweden reduced crime rates of the targeted generation and their sons by comparable amounts. The second policy evaluated in this thesis is a liberalization of the opening hours of the Swedish alcohol monopoly outlet stores that took place between 2000-2001. This study distinguishes itself from existing studies by mapping out an age-specific policy impact on crime for all ages and for a broad set of types of crimes. Whether and how alcohol policies shift criminal outcomes differently for different ages and type of crimes is not well established. The liberalized opening hours of outlet stores had very heterogeneous effects on crime by age and type of crimes. It reduced overall crime rates for male teenagers by 15-20 percent, mainly driven by reductions in drugs and property offences. Men in their mid-thirties also experience a substantial reduction of overall crime rates by 9 percent that comes from reductions in other crimes category and traffic crimes. While a strong increase of 10 percent in the crime rate for men in their early to mid-twenties can be mainly attributed towards a large increase in drug offences.

Keywords: Economics of crime; public policy; compulsory education reform; intergenerational transmission; returns to education; returns to human capital; age-crime profiles; alcohol policy

JEL Codes: I18, I20; I21; I28; J18; J62; K14; K42; H75

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# Introduction

Crime imposes large costs on society and public policies to reduce crime are various. From the criminal justice system side, increased police resources or harsher punishments have been considered as major tools to combat crime. While police and prison might appear to be the obvious policy tools, it is now debated in the literature whether alternative policies that are more cost effective and at the same time improve other outcomes should be considered. Education policy has been identified to be such an alternative policy tool, see Lochner (2011). Two earlier papers by Lochner and Moretti (2004) and Machin, Marie, and Vujić (2011) study the relation between compulsory schooling laws and criminal behavior. Lochner and Moretti (2004) demonstrate the crime reducing effect of education on crime using the increase of mandatory years of schooling in the US on arrest and imprisonment rates. Machin, Marie, and Vujić (2011) compare criminal behavior of the cohorts just before and just after the implementation of the comprehensive school system in Britain. These findings are important because they show the broader impact of educational reform and a way of improving outcomes for adults, beyond deterrence and punishment.

More generally, the links between economic incentives and crime have been established both theoretically and empirically in earlier studies. A prominent example is Freeman (1999) who outlines an economic model of crime where the choice between criminal and legal activity is determined by comparing the expected utility of each. Grogger (1998), Gould, Weinberg, and Mustard (2002), Machin and Meghir (2004), and Edmark (2006) demonstrate the importance of wages and labor market opportunities in driving crime. One implication of this is that improved education may reduce crime.

A more theoretically based approach was offered by Lochner (2004) who develops a life cycle model of education and crime and estimates a negative education-crime relationship. A study, based on this human capital approach by Williams and Sickles (2002) finds that years of schooling has a significant negative effect on crime in adulthood, and that there is a relationship between crime and other measures of human capital. Earlier studies support this empirical evidence on the education-crime relationship. For example, Freeman (1996) states for the 1991 US Census that two thirds of US prison inmates are high-school drop-outs and 12 percent of 24-35 year old high

school drop outs were incarcerated in 1993.<sup>1</sup>

The first chapter of this thesis empirically explores the impacts of a major education reform in Sweden on crime on the directly targeted generation. The Swedish compulsory school system was gradually introduced from 1949 to 1962 in municipalities by cohort of pupils. It was originally studied by Meghir and Palme (2005) for its effects on education and earnings. As shown in Meghir and Palme (2005) the reform significantly increased the number of years of schooling as well as labor earnings of those individuals who went through the new school system, in particular for individuals originating from homes with low educated fathers, i.e. not more than statutory level of schooling. This thesis first establishes that this reform substantially decreased crime rates of the target generation, in line with the previous literature on education reforms and crime. The effects of the reform on the sample of men whose complete conviction history we are able to observe starting from age 18-21 onwards is a reduction in overall crime by 1.5 percentage points. The analysis separated by age at offence, emphasizes that the effect is strongest at younger ages for convictions between the ages of 20-24. Furthermore, the overall effect is mainly driven by a reduction in property and severe traffic crimes. The reform also had a substantial negative effect on being repeatedly convicted.

An outstanding question is to what extent education policies have long term effects on criminal behavior in the sense that they also affect criminal behavior of the children of those directly affected by educational reforms. The second chapter moves on to this novel contribution and analyses the reform effects of parents on their son's crime. There are good reasons to expect so, considering the strong intergenerational correlations in criminality and the fact that education policies can affect parental resources as well as skills important for parenting. This is associated with the more general question of whether policy can change the intergenerational transmission of human capital and offer a way of breaking the cycle of poverty.

Intergenerational associations of criminal behavior have been documented in the literature. In the Swedish context Hjalmarsson and Lindquist (2012) document a strong correlation between crime of fathers and children of both genders using the Stockholm Birth Cohort Study. In a second Swedish study the same authors Hjalmarsson and Lindquist (2013) focus on parent-child correlations in crime using adop-

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<sup>1</sup>This negative correlation between crime and education has also been documented in the criminology and sociology literature, for example Sabates and Feinstein (2008a).

tion data, to determine the factors through which mothers and fathers influence child criminality, which follows the approach of Björklund, Lindahl, and Plug (2006). From the economics point of view this question relates to the investments that parents make in their children and the way that parental education may affect such investments, see Becker (1981).

The second chapter first establishes that there exist a large intergenerational correlation between fathers and sons crime, and between fathers education and sons crime. Building onto the evaluation of the Swedish comprehensive school reform of the first chapter it then shows that the reform also substantially reduced crime for sons whose father went through the new school system. The effects are large with a 0.8 percentage points decrease in crime rates for sons whose father was exposed to the new school system. The effect is mostly driven by a reduction in crime at early ages 15-19. The crime types mostly affected are violent crimes, traffic crimes and fraud. Furthermore, the effect is only present if the father was affected by the reform - not the mother. The chapter proceeds by showing that home environments (parental earnings, education, parental quality match) improved for sons in families where the father was exposed to the reform. It must be emphasized that these improvements in several dimensions of the home environment cannot be interpreted as underlying mechanisms through which crime rates were reduced. Though, they are in line with theories of intergenerational transmission of human capital (Cunha and Heckman (2007)) or theories from sociology on the formation of social capital (Coleman (1988)).

The first two chapters show that education reforms can have strong and long lasting effects on crime. It establishes that own education can reduce crime, but also shows an intergenerational effect of such education policy. But, there are other important dimensions such as circumstances and temporary influences that are likely to play a key role in criminal activity. Alcohol consumption is one such important contributing factor for example to promote violent crimes. General statistics in the US report that about 33 percent of state prisoners self report to have been under the influence of alcohol at the time of the offence in 2004 (see Bureau of Justice Statistics (2004)), with even higher shares reporting alcohol influence for violent crimes and public disorder. Similar statistics in England and Wales show that victims of violence report in 47 percent of the cases that the offender was perceived to be under the influence of alcohol (see Crime Survey for England and Wales (2013)). While, these correlations cannot be seen as causal pathways, they still point towards alcohol to be a

key determinant in crime. Worldwide the importance of alcohol in criminal activity is recognized by governments leading to a wide range of crime prevention policies, such as Minimum Legal Drinking Age Laws, mandatory opening hours of bar and alcohol outlet stores, etc. A large set of studies is concerned with estimating the impact of such alcohol policies on crime, summarized in the survey of Carpenter and Dobkin (2010). For example, convincing evidence on targeted sales restrictions, the type of policy evaluated in the final chapter, is presented in Biderman, De Mello, and Schneider (2010). They document a large decline in homicides and battery following the mandatory closing hours of bars in the São Paulo Metropolitan Area. In the Swedish context a previous study by Grönqvist and Niknami (2011) finds increased property crime rates and no effect on violent crimes following the changes in the Saturday opening hours in the Swedish state monopoly alcohol stores for men around the age of 20, which is the legal limit to purchase alcohol in store. Minimum Legal Drinking Age Laws have been used extensively, for example to show increased mortality rates, motor vehicle deaths, or alcohol related deaths at the age cut-off (see Carpenter and Dobkin (2009)). Also studies concerned with the question of complementarity versus substitutability between alcohol and drugs have exploited MLDA laws in the US (see Yörük and Yörük (2011), and opposing results in Crost and Guerrero (2012)).

The final chapter of this thesis moves on to evaluating a public policy that can contribute towards the circumstantial dimension of crime. The aim is to better understand the link between alcohol availability, age and different crime types. Evidence on the well established age-crime profiles shows that crime varies substantially across ages and types of crimes which implies that the effects of alcohol on crime are likely to vary by age and types of crimes too (Farrington (1986), Moffitt (1993), and Sampson and Laub (2003)). It expands the existing literature with a thorough analysis of how alcohol policy can reduce/increase crime by types of crimes and most importantly for all age-ranges, by being able to map out an age-profile for a broader set of crime categories. Thereby, it contributes towards the more general discussion of how alcohol policy can be used as policy tool to influence crime, and more specifically towards the existing literature using temporal restrictions as identifying strategy.

In 2000-2001 the Swedish government performed a policy experiment that expanded opening hours of the alcohol monopoly stores to open on Saturdays. I exploit this liberalization of alcohol availability to analyse how crime changes by age and type of crime. The empirical analysis is based on a difference-in-differences approach ex-

exploiting the staggered implementation of the policy across Swedish counties and time. I find that alcohol sales (in pure alcohol sold per capita) increased by 6.6-10 percent. For overall crime rates by age I find that the liberalized opening hours reduced crime rates of men between the ages of 16-17 and 30-40. While, for two other age groups, crime rates increased significantly: For men in their twenties (20-28 year old men) and for middle aged men (45-55 year old men). The findings are robust in particular for the younger age groups. The overall results appear to be mainly driven by drug crimes, traffic crimes, property crimes and fraud.

All empirical analyses in this thesis exploit various data sources from Sweden. The empirical work is primarily based on individual register conviction data that cover all convictions in Sweden between 1973-2010. This data was provided by *The Swedish National Council for Crime Prevention (Brå)*. For each individual conviction in Sweden during that time the data contains an individual identifier, the age of the offender at the time of the offence, the date of the conviction, and the date of crime committed, regional court identifiers, the number of crimes within each individual conviction, and several variables that allow me to identify the type of the main crime for which the individual was convicted (in a particular conviction). The type of crime variables are based on the Swedish law books, sections of the law, paragraphs etc. through which I define seven crime categories: violent crimes, property crimes, fraud and tax offences, drug crimes, traffic violations that exclude speeding or parking tickets, sex crimes, and a category others that includes all other crimes that cannot be categorized into one of the six. The data contain all crimes that individuals are subsequently convicted for and the type of crime categorization is only limited by being able to uniquely identifying a type of crime within the law book (for example assault of a husband towards his wife can not be distinguished between assault towards a stranger, both offenders will be convicted based on the same laws concerning assault). For chapters 1 and 2 the multi-generation register was used to link family connections across three generations. This means we can link parental reform status to each child in the considered sample, parental education and grandparent education. In addition individual earnings and education data were used and matched on to the crime data. For chapter 3 additional aggregated labour market data and population data come from Statistics Sweden. The evaluation of the alcohol policy on alcohol sales is performed based on alcohol sales data provided by the Swedish alcohol monopoly

store *Systembolaget*. The details of the relevant data sources and how the data was constructed is provided in each chapter.

Overall, this thesis exploits extensive administrative data over a very long horizon and for the entire Swedish population including various additional data sources to analyze how public policies can affect criminal behaviour. The focus lies on two potentially very important determinants of crime - education and alcohol consumption.

# Chapter 1

## Education Policy and Crime

### 1.1 Introduction

Earlier papers have demonstrated that crime and education are related and that policies that increase education can reduce crime (see Lochner and Moretti (2004)). There are strong theoretical reasons why this should be the case since increased education improves economic opportunity and can also increase the psychic costs of committing crimes. Becker (1981), Freeman (1999) and Lochner (2004) amongst others have developed theoretical models with these predictions. A number of papers have demonstrated the empirical relevance of these models.<sup>1</sup>

We use data containing individual information on all convictions and prison sentences in Sweden between 1973 to 2010, including details on the types of crimes committed. The dataset also links information on three generations. This allows us to estimate the effect of the reform on both the parent generation (confirming results from earlier studies) and on the child generation, by age and by type of crime, while conditioning on the education level of the grandparent generation. This chapter presents the results on the parent generation. Our empirical approach is based on comparing changes in the crime rate across cohorts in municipalities that implemented the reforms at different times.

Two earlier papers by Lochner and Moretti (2004) and Machin, Marie, and Vujić

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<sup>1</sup>Examples include Grogger (1998), Gould, Weinberg, and Mustard (2002) Machin and Meghir (2004) and Edmark (2006). For Sweden Edmark (2006) shows the relationship between unemployment rates and property crimes on county level. Williams and Sickles (2002) finds that years of schooling reduces crime in adulthood. Freeman (1996), based on the 1991 US census, documents that 12 percent of 24-35 year old high school drop outs were incarcerated in 1993; The criminology and sociology literature presents similar evidence, see e.g. Sabates and Feinstein (2008a) and Sabates and Feinstein (2008b). Finally, Gallipoli and Fella (2008) develop an empirical model that allows for the evaluation of policies designed to reduce crime allowing for general equilibrium effects.

(2011) respectively study the relation between compulsory schooling laws and criminal behavior. Lochner and Moretti (2004) use changes in compulsory schooling laws across time between US states to identify the effect of increasing education on crime. Exploiting the implementation of the comprehensive school system in Britain Machin, Marie, and Vujić (2011) compare criminal behavior of the cohorts just before and just after the implementation to show similar crime reducing effects of increased education.

In a recent paper Hjalmarsson, Holmlund, and Lindquist (2011) followed up on our working paper (Meghir, Palme, and Schnabel (2011)) by extending the observation window for convictions back to 1973 and obtaining data on crime by type; they then focus on using the reform as an instrument for the impact of education on crime for the parent generation only, i.e. for those who were directly affected by the reform - they do not consider intergenerational effects. Their approach requires the strong assumption that all the impact of the reform is mediated by the change in years of education. Below we argue that this assumption is unlikely to hold, given the nature of the reform. Since our original publication we have also extended our data back to 1973 and forward to 2010 obtaining also data on type of crime for *both* generations we consider. The focus of this chapter is the impact of the reform itself on those directly targeted by the reform and we are also able to investigate heterogeneity of effects with respect to socioeconomic status of the previous (grandparent) generation.<sup>2</sup>

This chapter is organized as follows. We first provide a brief description of the reform followed by a data section outlining our administrative data, documenting the crime rates and presenting descriptive evidence. We then discuss our empirical strategy followed by the results on the parent generation.

## 1.2 The 1950 Swedish Education Reform

### 1.2.1 The Reform

Prior to the implementation of the comprehensive school reform, pupils attended a common basic compulsory school (*folkskolan*) until grade six. After the sixth grade pupils were selected to continue one or (mainly in urban areas) two years in the basic compulsory school, or to attend the three year junior secondary school (*realskolan*). Selection of pupils into the two different school tracks was based on their past grades.

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<sup>2</sup>We argue in the paper that the reform cannot be used as an instrument for education in either generation and as a result we focus on the direct effect of the reform on both generations.



The pre-reform compulsory school was in most cases administered at the municipality level. The junior secondary school was a prerequisite for the subsequent upper secondary school, which, in turn, was required for higher education.

In 1948 a parliamentary school committee proposed a school reform that implemented a new nine-year compulsory comprehensive school, abolished early tracking and introduced a national curriculum. Until the age of 16 all children would henceforth attend a comprehensive school with a national curriculum. The extension to nine years of compulsory schooling meant that in many parts of the country the compulsory increase was two years, while in others it was one.<sup>3</sup>

The reform is a combination of various components, all of which have been elements of reforms in other countries. For example, England increased compulsory school leaving age in 1973 from 15 to 16 and abolished selection at 11, gradually creating comprehensive schools starting in the mid-sixties.<sup>4</sup> In the early eighties England also adopted a common curriculum. Thus, the effect of these reforms is of general interest in itself and showing an impact on crime, even as a package, can be of broad interest.

If we could disentangle the impact of each component of the reform we could learn more. Unfortunately, with the current design this is not possible as they were all implemented together. And while different groups based on socioeconomic status may be affected differently by the various components, spill-over effects will ensure that all are impacted as a result of the different components. For example, increasing compulsory schooling and abolishing tracking would change the social mix in schools and dilute the resources available per child. Indeed, this is a key reason why the reform cannot be safely used as an instrument for years of education.

### 1.2.2 The Social Experiment

The proposed new school system, as described above, was introduced gradually from 1949 to 1962 in municipalities or parts of city communities, which in 1952 numbered 1,055 (including 18 city communities).<sup>5</sup> The selection of municipalities was not random. However, the selection of areas was guided by an attempt to ensure the

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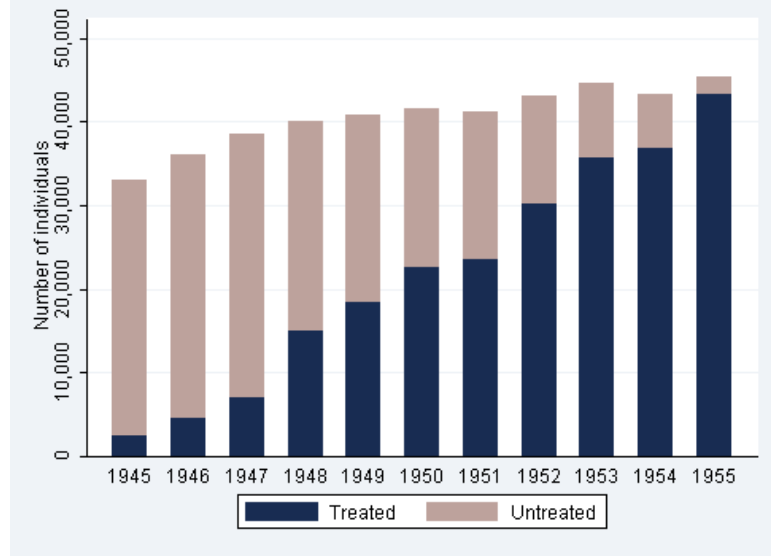
<sup>3</sup>The school reform and its development are described in Meghir and Palme (2003), Meghir and Palme (2005), and Holmlund (2007). For more detailed reference on the reform, see Marklund (1980) and Marklund (1981).

<sup>4</sup>Some parts of England still have selection, e.g. Kent.

<sup>5</sup>This was done for evaluation purposes as well as a way of resolving the political differences relating to the reforms. The official evaluation National School Board (1959) was mainly of administrative nature. Details on this evaluation are also described in Marklund (1981).

implementing municipalities were representative of the whole country, both in terms of demographics as well as geographically. Given the design of the social experiment our approach will be based on a difference-in-differences estimation strategy.

Figure 1.1: Number of Individuals in Sample Assigned to the Reform



When a municipality introduced the new school system it implemented it either for the cohort of pupils who were in fifth grade at the time of the decision or for those who were currently in the first grade, effectively delaying the start of the program. In our analysis we consider cohorts born between 1945 and 1955. Figure 1.1 shows the number of observations in our sample in each year birth cohort and the proportion of the parent generation assigned to the reform.

As mentioned above, the reform was not implemented randomly across municipalities. Both the central government and the local authority had a say on whether and when the reform would be implemented. In the empirical analysis that will follow we will be controlling for municipality fixed effects and other characteristics that vary over time to allow for permanent and potentially confounding characteristics that may differ across early and late implementers. Nevertheless, it is interesting to document here how these municipalities differed. Thus, we run a regression of the earliest cohort for which a municipality implemented the reform on three municipality characteristics that are potentially correlated with the municipality crime rate: population size, average income and tax rate in 1960, when the reform could not have any effects on outcomes. The results shown in Table 1.1 imply that early implementers were higher income and had a higher local tax rate. The municipality population size had no effect.

Table 1.1: Timing of Implementation and Municipality Characteristics

Dependent variable: first cohort implemented		
	(1)	(2)
	all municipalities	excluding Stockholm
Population in 1960	0.036 (0.039)	0.083 (0.074)
Income level in 1960	-0.072*** (0.012)	-0.074*** (0.012)
Tax rate in 1960	-0.654*** (0.066)	-0.662*** (0.067)
Observations	984	983

Notes: Significance levels \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . The dependent variable is the cohort when the reform was first implemented in the municipality, the regressors are municipality population size, average income and tax rate in 1960.

### 1.3 Data Sources and Descriptive Statistics

We use a sample originally obtained from Sweden’s population census. To link individuals across generations we used the multi-generation register, provided by Statistics Sweden.<sup>6</sup> We are able to link and use three generations in our analysis: the *parent generation* consisting of all individuals born in Sweden between 1945 and 1955, who attended school during the social experiment described above; their parents labeled as the *grandparent generation*; and their children referred to as the *children generation*.<sup>7</sup>

We do not have direct information on individual assignment to the reform. Our reform assignment variable is based on information on parish of birth from the population census. Using information on year of birth and when the individual’s municipality of birth implemented the reform we then use an algorithm provided by Helena Holmlund (see Holmlund, 2007) to decide whether or not the individual went through the pre or post reform school system.

The advantage with using this variable for reform assignment, rather than one based on direct information on type of school attended, is that it is not susceptible to endogeneity caused by parents moving to municipalities on the basis of preferences for school system for their children. The disadvantage is that it might lead to some attenuation of the effects of the reform because some individuals may have moved leading to some measurement error with respect to actual assignment.

Fortunately, we can investigate this by deriving a reliability ratio (see Aigner (1973)); For a subset of the data set - those born the 5th, 15th or 25th each month

<sup>6</sup>See Statistics Sweden (2003).

<sup>7</sup>Even though we have information on biological and adoptive parents and children, we exclude all individuals who have been adopted, or who have adopted children themselves.

in 1948 or 1953<sup>8</sup> we have register-based information on which school system they actually were assigned to. For 87.3 percent of this sample we were able to match information on actual reform assignment and the one predicted by the municipality of birth - which is what we use in this study. The discrepancy between the two measures is only 9.9 percent: 5.3 percent moved to a non-reform municipality and 4.6 percent moved in the other direction. This implies that on average our estimates will be attenuated by a factor of 0.901 with respect to correct assignment to the reform.<sup>9</sup>

Data on all convictions in Sweden covering the time period between 1973 and 2010 is provided by the Swedish National Council for Crime Prevention (Brå) and has been linked to individuals in our data set using the unique personal identifying number. This means we are able to link individuals to actual convictions, which is an advantage of our study compared to previous studies of the effects of education reform on criminal behavior (Lochner and Moretti (2004) and Machin, Marie, and Vujić (2011)). For each conviction we have detailed information on the type of crime for the main violation within the conviction and the age when it was committed. We categorize crimes into seven types: violent crimes, property crimes, fraud and tax evasion, traffic crimes, drug and trafficking violations, sex crimes and others containing crimes that cannot be categorized as any of the latter six categories. The traffic crimes need to be serious enough to lead to a court case and do not include speeding and parking offenses.<sup>10</sup>

We select the sample of men born 1945-1955 who were alive in 1973 (when the crime records begin) and who had not migrated out of Sweden permanently. Information on the education level for the *parent generation* and *child generation* was obtained and matched onto our sample from the Swedish National Education Register. From the education census we also link in the education of their fathers (the grandparent generation), which is available if they were younger than 60 in 1970, i.e. for 71.6 percent of the cases.

Under the column "Total", Table 1.2 shows the overall conviction rate for men in

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<sup>8</sup>These are included in the so called UGU-data set, collected by the Department of Education, Gothenburg University - see Meghir and Palme, 2005.

<sup>9</sup>The attenuation coefficient is  $Pr(R = 1|R^B = 1) - Pr(R = 1|R^B = 0) = 0.947 - 0.046$  where  $R$  denotes actual reform assignment and  $R^B$  reform assignment based on municipality of birth.

<sup>10</sup>Types of crimes are detailed in several variables that specify the chapter, paragraph, moment, piece and point in the section of the relevant penal code (law-book). Details of the types-of-crime variables in the conviction data are in Brå Variabelbeskrivning Lagföringsregistret (2009) and the documentation of coding crime types can be found in Brå Kodning av Brott (2010). The crime register also contains information on the number of crimes within each individual's conviction, the date of conviction, the age of the offender, as well as the penalty for each crime.

the parent generation. We also report the conviction rate by type of crime *conditional* on being convicted in the remaining columns. Since individuals can have multiple convictions these columns add up to more than 100. The conviction rates corresponds to the observation period 1973-2010. During the entire observation period 1973-2010, 34 percent of men in the parent generation have been convicted at some point of a crime. Of those convicted 58 percent were convicted of a traffic crime (serious enough to lead to a court case), 21 percent of a property crime, 18 percent for fraud (including tax evasion) and 33 percent of "other" unspecified crimes. One in five convictions led to a prison sentence. Additional support of such high conviction rates in Sweden is provided by other Swedish studies that have shown similar conviction rates, see Hjalmarsson and Lindquist (2012), Hjalmarsson and Lindquist (2013), and Grönqvist (2011). In Appendix Table 1.8 we separately report conviction rates for individuals from a lower socio-economic status (SES) background, which in the entire paper refers to those individuals for whom the grandparent generation had just pre-reform statutory level of education. Their conviction rates are only slightly higher. This surprising result might be due to the fact that the low SES group represents 63 percent of the population.

Table 1.2: Conviction Rates by Age and Types of Crimes for Parent Generation

	Percent convicted by crime types conditional on being convicted								
	Total	Violent	Property	Fraud	Traffic	Drugs	Sex	Others	Prison
<b>Panel A: Men in Parent Generation: at least one conviction</b>									
All	33.94	14.97	21.02	18.16	57.96	17.04	1.623	33.23	19.65
ages 20-24	19.11	10.12	25.66	11.82	43.20	17.75	0.409	30.88	16.29
ages 25-29	11.53	10.54	21.89	14.75	39.35	20.57	0.669	28.60	20.82
ages 30-39	12.68	13.93	19.06	19.84	43.84	15.93	1.252	28.85	21.44
ages 40-49	11.21	13.36	13.49	13.62	54.92	9.350	1.523	24.17	
<b>Panel B: Men in Parent Generation: multiple convictions</b>									
All	15.75	25.40	34.79	30.03	71.06	24.98	2.518	49.17	35.54
ages 20-24	6.399	18.66	48.30	24.26	52.82	26.13	0.751	48.04	35.43
ages 25-29	3.620	19.99	42.35	29.79	49.89	29.29	1.097	43.41	43.71
ages 30-39	4.313	26.12	37.74	35.04	56.54	24.55	1.902	43.65	44.26
ages 40-49	3.309	25.00	29.65	24.56	64.23	18.84	2.175	38.65	

Notes: Table shows overall conviction rates (Total) and type of crimes rates conditional on having been convicted at least once or twice. The sample are men with all SES. For the age specific conviction rates only men who are fully observed for the relevant age bracket are included. For Panel A and B: All includes the whole sample of men born 45-55 (N=447,382) and the conviction rates refers to having ever been convicted or having been convicted at least twice between 1973-2010. Age specific conviction rates for men in Parent Generation includes: for ages 20-24 cohorts 53-55 (N=133,200), for ages 25-29 cohorts 48-55 (N=339,888), and for ages 30-39, and 40-49 the whole sample of cohorts 45-55 (N=447,382).

The Table also reports conviction rates by age. Comparing the crime rates of the parent generation across age groups<sup>11</sup> shows that conviction rates decline with age,

<sup>11</sup>Note the 5-year age ranges defined for men below 29, versus the 10-year age ranges for men above 30.

which applies to all types of crimes. This is best seen in Figure 1.2 for the parent generation, which shows a pronounced decline of crime with age.

Figure 1.2: Age Profile of Crime Rate for the Parent Generation by Cohort

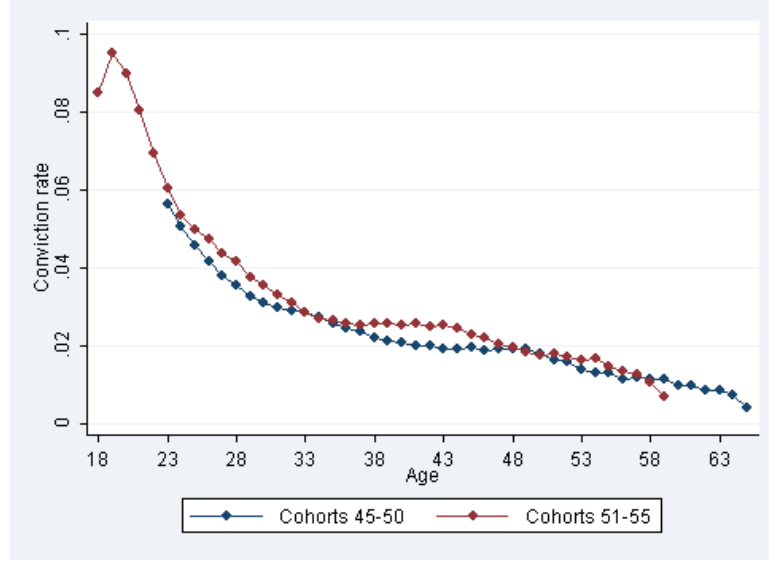


Table 1.2 also provides information on more serious criminal activity, namely convictions leading to prison and multiple (2+) convictions, as well as the percent convicted of each type of crime conditional on having 2+ convictions. The distribution by type of crime involves more violent, property and fraud crime, although traffic related crimes are also prevalent. About 20 percent of those convicted in the parent generation were incarcerated. Moreover, when we consider multiple convictions the prevalence of violent, fraud and property crimes increases.

In Appendix Table 1.10 we report the crime rates for women. These are almost a quarter of the male rates. As with men there is no difference in the crime rates when we focus on the low SES background. The reform has no discernible effect on these rates and we present results in the Appendix for completeness. However, we do examine whether exposing the mother to the reform affects the crime rate of sons in Chapter 2.

## 1.4 Empirical Strategy

The main outcome variables we use is whether an individual was ever convicted during the observation window 1973-2010 for any crime and by type of crime; and whether an individual was convicted at certain ages: 20-24, 25-29, and 30-39 for the parent generation. We present results for the whole sample and separately for those with a

low SES background.

Our outcome variable is based on convictions and incarcerations, rather than offending as such. By the administrative nature of our data this is what we observe. The interpretation of our results presumes that the impact on convictions reflects a real reduction in crime and not simply an improved ability by criminals to evade arrest and convictions. So a key assumption in this approach is that the reform did not affect the relationship between offending and convictions.

The crime records start in 1973 and the gradual transition to the new school system covers the cohorts born between 1945-1955. As documented above, most crimes are committed by young people and it is this age group that is most likely to be affected by the reform. Thus, for the parent generation we estimate impacts on crime for the 1952-55 cohort who were 18-21 years old in 1973 when our records begin. They are followed up until 2010 when they are 55-58. The crime rate of females is very low - about a quarter of the male one, and was not affected by the reform; so we include the results for women in an Appendix for completeness but do not discuss the results at depth.

Since the reform was not randomized we control for potential differences across treatment and control municipalities using a difference in differences approach. This compares the change in the crime across cohorts in municipalities that implemented the reform for the younger cohort but not the older one to the change in crime rate across the same cohorts living in municipalities where there was no change in policy for these same cohorts. In practice we do this for all considered cohorts in our window and all 1000 or so municipalities. Thus our approach is best described by the regression

$$(1.1) \quad y_{i,m,t}^* = \alpha + \beta_1 R_{i,m,t} + \gamma_1' t_i + \gamma_2' M_i + \epsilon_{i,m,t},$$

where  $y_{i,m,t}^*$  is the latent crime "intensity" outcome observed for person  $i$  born in municipality  $m$  and in birth cohort  $t$ . A conviction corresponds to  $y_{i,m,t}^* > 0$ .  $R_{i,m,t}$  is the reform indicator, which equals one if individual  $i$  belongs to a municipality and cohort that has been assigned to the new school system;  $t_i$  is a vector of indicator variables indicating to which cohort individual  $i$  belongs to and  $M_i$  is a vector of indicator variables indicating in which municipality individual  $i$  was born.  $\epsilon_{i,m,t}$  is conditionally independent of  $R_{i,m,t}$ . The general assumptions underlying the method

of Difference in Differences whether unconditional or conditional on explanatory variables (like here) are discussed in Heckman and Robb (1985), Heckman, Ichimura, Smith, and Todd (1999) and Athey and Imbens (2006).

Based on the latent equation above we use the linear probability model (LPM), which we estimate by GLS. The main reason for this specification is computational convenience: there are about 1,000 municipality and 4 (or depending on specification up to 11) cohort fixed effects. To check whether using a LPM biases the results we ran a Monte Carlo experiment replicating the crime rates across municipalities and imposing an average effect of the reform equal to what we obtain. Assuming the data was generated by a normal probability model (probit) and then using a LPM only biased the results upwards by 5 percent with respect to the true average effect - a difference that is statistically indistinguishable in our data. In what follows all regressions include a full set of fixed effects for the birth municipality and the cohort of the parent generation, as well as the education level of the previous (grandparent) generation. All standard errors are clustered at the municipality level.

## 1.5 The Reform, Educational Attainment and Crime in the Parent Generation

### 1.5.1 Education

Table 1.3 shows the estimates of the effects of the education reform on years of schooling for the parent generation.<sup>12</sup> The results are presented for all men and all women born between 1945 and 1955, as well as separately by SES background.

The reform significantly increased years of schooling of both men and women and substantially more so for the former. The overall effect is larger for the low SES group. We also find a small but significant effect (at the 10 percent level) on the men with higher educated fathers. We find no effect on those women from a higher SES background.<sup>13</sup>

In our analysis of crime that follows we show results for the whole sample and

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<sup>12</sup>Years of schooling are inferred from the level of schooling attainment obtained from the registers.

<sup>13</sup>In Appendix Table 1.13 we also show results with municipality specific trends - this does not lead to any large or significant changes. Comparing with Meghir and Palme (2005) the effects are overall slightly different. However, these estimates relate to a larger group of cohorts - not 1948 and 1953, and the results might be attenuated by a factor of 0.9 because we use municipality of birth instead of actual reform assignment.



Table 1.3: Reform Effects on Years of Schooling for Parent Generation

Dependent variable: Own years of schooling			
	(1)	(2)	(3)
Sample:	All SES	Low SES	High SES
Panel A: Men born 45-55			
Reform	0.174*** (0.038)	0.267*** (0.038)	0.052* (0.030)
Obs	444,272	282,080	162,192
mean years of schooling	11.62	10.91	12.85
Panel B: Women born 45-55			
Reform	0.108*** (0.033)	0.161*** (0.029)	0.051 (0.032)
Obs	423,781	268,567	155,214
mean years of schooling	11.75	11.14	12.80

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. The dependent variable is number of years of own schooling. Robust standard errors, clustered by municipality of birth, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of individual. Column (1) includes father's education levels.

for the low SES group separately. We do not show results for the high SES group separately because there is insufficient precision to draw clear conclusions.

## 1.5.2 Crime

This section reports results for the parents, providing a link with the existing literature and establishing that the reform did indeed have a direct effect on crime.<sup>14</sup>

Table 1.4: Reform Effects on Crime by Types of Crimes for Parent Generation

	(1)	(2)	(3)	(4)	(5)	(6)
	Any	Violent	Property	Drugs	Traffic	Fraud
Panel A: Men born 52-55, All SES, Obs: 176,232						
Reform	-1.464*** (0.556)	-0.364 (0.297)	-0.783** (0.347)	-0.427 (0.294)	-1.391*** (0.473)	-0.334 (0.269)
$\bar{y}$ %	38.62	6.156	9.366	7.233	22.59	7.024
Panel B: Men born 52-55, Low SES, Obs: 107,557						
Reform	-1.693** (0.680)	-0.344 (0.357)	-0.757* (0.421)	-0.409 (0.361)	-1.899*** (0.604)	-0.490 (0.355)
$\bar{y}$ %	40.00	6.946	10.09	7.291	23.66	7.438

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between 1973-2010 for any crime in column (1), or one of the crime types specified in columns (2)-(6). Robust standard errors, clustered by municipality of birth, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of individual. Panel A also includes father's education levels.

In Table 1.4 we present the effects of crime on the parent generation for cohorts

<sup>14</sup>See Lochner and Moretti (2004).

1952-55, who are aged 18-21 at the start of our observation year in 1973. Descriptive statistics for these cohorts are presented in Appendix Table 1.9. The crimes we observe are always after the end of statutory schooling and hence the effects do not include the more mechanical effect of keeping children off the streets. The overall effect of the reform is to reduce by 1.46 percentage points the probability of a conviction over the entire observation period - the effect is highly significant. When we keep only the low SES individuals the impact increases to 1.69 percentage points but the difference is not significant.

In the remaining columns we split up the effect by type of crime committed. Here it becomes clear that the impact is driven by property crime, which decline by 0.78 percentage points and traffic crimes, which decline by 1.39 percentage points. Note that the impacts by type will typically add up to more than the total effect, because many individuals commit more than one type of offense.

In the descriptive statistics it became obvious that younger people have much higher crime rates; it is thus reasonable to expect the impact of the reform to be concentrated at younger ages. Indeed this is the case as we show in Table 1.5: the effect for the 20-24 age group is -0.988, declines slightly for the 25-29 age group and becomes much smaller and insignificant for the 30-39 year olds of the same cohort (although the estimates are not significantly different from each other).

Table 1.5: Age Specific Reform Effects on Crime for the Parent Generation

	(1)	(2)	(3)
Convicted at age:	30-39	25-29	20-24
Panel A: Men born 52-55, All SES, Obs. 176,232			
Reform	-0.347 (0.392)	-0.890** (0.386)	-0.988** (0.448)
$\bar{y}\%$	12.38	11.61	18.65
Panel B: Men born 52-55, Low SES, Obs. 107,557			
Reform	-0.508 (0.475)	-0.657 (0.485)	-1.045* (0.565)
$\bar{y}\%$	13.19	12.28	19.79

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 30-39, 25-29, and 20-24. The sample are men from the parent generation born 52-55. Robust standard errors, clustered by birth municipality, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of the individual. Panel A also includes father's education levels.

We also estimated the effect of the reform on women. We found no discernible effects. The complete set of estimates are presented in Appendix Tables 1.11 and

1.12.

It is interesting to estimate the effect of the reform on more serious criminal activity, since this will be associated with larger social costs. To get at this we consider impacts on recidivism, i.e. on the probability of having two or more (2+) separate convictions as well as convictions that lead to a prison sentence. Table 1.6 shows that the impact of the reform on having 2+ convictions is very strong, reducing them by 1.48 percentage points overall and by 1.77 percentage points for the low SES group. When we consider the effects by age (in the next three columns) we get a strong effect for the youngest group, which is higher for the low SES individuals. We also seem to get a marginally significant effect for the older 30-39 age group.

Table 1.6: Reform Effects on Recidivism and Incarceration for Parent Generation

Dependent variables:	Multiple convictions/Recidivism				Incarceration
	(1)	(2)	(3)	(4)	(5)
Convicted at age:	All	30-39	25-29	20-24	All
<hr/>					
Panel A: Men born 52-55, All SES, Obs. 176,232					
Reform	-1.476*** (0.491)	-0.422* (0.227)	-0.344 (0.233)	-0.896*** (0.290)	-0.488 (0.316)
$\bar{y}\%$	18.88	4.422	3.824	6.206	7.507
<hr/>					
Panel B: Men born 52-55, Low SES, Obs. 107,557					
Reform	-1.767*** (0.566)	-0.409 (0.293)	-0.284 (0.283)	-1.053*** (0.339)	-0.580 (0.366)
$\bar{y}\%$	20.07	4.849	4.192	6.889	8.211

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having been convicted at least twice between 1973-2010 (column (1)) or between the ages of 30-39, 25-29, and 20-24 (columns (2)-(4)), or an indicator variable for having ever been convicted for a prison sentence between 1973-2010 in column (5). The sample are men from the parent generation born 52-55. Robust standard errors, clustered by birth municipality, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of the individual. Panel A includes father's education levels.

Only 30 percent of convictions end up in prison sentences for the parent generation (incarceration rate 7.5 percent). At the tails of the distribution the linear probability model may not be a very good approximation. So we estimated the effects in two different ways; first we use the LPM. Second, we use a probit, using a reduced set of fixed effects: we group the municipalities by the first cohort for which they implemented the reform and define a fixed effect for each of these groups. This probit gives almost identical results to the LPM and hence we report results from the latter.<sup>15</sup> The LPM results are presented in Table 1.6. The estimates are negative and quite large implying a reduction in prison by about 0.49-0.58 percentage points (st. error

<sup>15</sup>For example for the 52-55 cohort of the adult generation the impact with the probit is -0.497 (se 0.329), while with the LPM and a full set of municipality fixed effects we get -0.488 (se 0.316).

0.32-0.37). For those from a low education background the effects are significant at the 10 percent level.

## 1.6 Common Trends Assumption Parent Generation

One of the key identifying assumptions of our approach is that the underlying trends in crime are the same irrespective of the birth municipality of the parent generation. This section provides evidence for this assumption in two ways: first, we repeat our estimation assuming that the reform took place at a different date than it actually did (placebo estimations); second, we explicitly include municipality specific trends to evaluate whether they are significant (akin to a test of overidentifying restrictions) and whether our results are sensitive to their inclusion. For these tests we group municipalities by the earliest cohort for which they implemented the reform and we look for omitted trends specific to each of these groups.

For the placebo estimations, where we pretend that the reform was implemented later, we only use the sample of men born 52-55 who were treated by the reform. To construct placebo treatment and control groups we then pretend that the reform was implemented successively one or two years later. We (falsely) assign the first treated cohort (the first two treated cohorts or the first three treated cohorts) in each municipality group to be untreated and the remaining ones to the treated group. This provides two placebo estimates.

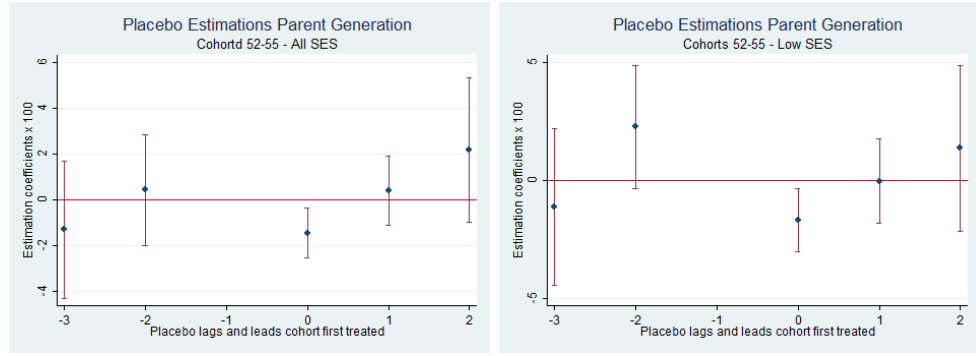
Similarly, for the placebo estimations where we pretend that the reform was implemented earlier, we restrict the sample to men born 52-55 who were not treated by the reform.<sup>16</sup> The placebo treatment groups are defined by (falsely) assigning the two last untreated cohorts or the three last untreated cohorts to the treated group and the remaining cohorts stay in the control group. This provides an additional two placebo estimates.

The results are all brought together in Figure 1.3. Each dot represents the estimate assuming the reform took place at the specified period on the x-axis (relative to when it actually took place, which is the zero point). The vertical line around the dot represents the 95 percent confidence interval. The graph shows that the largest (in absolute value) and only significant effect is obtained when we use the correct timing

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<sup>16</sup>We require at least two treated cohorts and one untreated cohort in each municipality group to implement the estimator. This means that we start our first placebo estimation pretending the reform was implemented two years earlier than it actually was.

Figure 1.3: Placebo Estimations Men 52-55



for the reform assignment (that is at zero). In all other cases we estimate insignificant effects and no particular pattern shows up implying there is nothing systematic taking place biasing the results towards an effect on crime.

For our second approach, Table 1.7 shows F-statistics and p-values for two tests: that the trends are not specific to groups of municipalities (sorted by cohort of first implementation) and that the impacts remain unaffected by the inclusion of such trends. We find no evidence that such specific trends are present. For completeness the parameter estimates obtained when these trends are included are shown in the Appendix in Tables 1.13 - 1.16. For example, the overall effect on any crime for the 52-55 men without any municipality specific trends is -1.464 (se 0.556) while when we include trends this becomes -1.292 (se 0.921). For the lower SES group the effect drops a bit from -1.693 (se 0.680) to -1.495 (se 1.053). None of these changes are significant. There is a loss in precision when the municipality specific trends are added. Some of the effects become larger and even significant when they were not before (e.g. the impact on drugs crime and the impact on crime between ages 25-29). Nevertheless, as shown, the differences are not significant and the overall conclusions do not change.

Table 1.7: Trends Tests for Parent Generations

Tests:	Joint test of trends=0		Reform parameter across models	
	(1)	(2)	(3)	(4)
	All SES	Low SES	All SES	Low SES
F statistic/chi2 statistic	0.804	0.803	0.066	0.062
P-value	0.690	0.691	0.797	0.804

Notes: Test 1 jointly tests the hypothesis that trends are common across municipalities. Test 2 tests the hypothesis that the impacts are the same when comparing the specification with and without trends.

## 1.7 Conclusions

The Swedish educational reform increased education for the men (and women) directly targeted by the reform, confirming the previous results of Meghir and Palme (2005) for a different sample of cohorts. This chapter established further, that the reform reduced the crime rates of men who were directly affected by the reform (the parent generation). Our analysis of this generation is limited by the fact that we start observing them from the age of 18-21 onwards. Thus, we are not able to map out a complete picture from the beginning of their criminal career, though for the selected cohorts, we are able to observe them for an age with highest criminal activity. The overall crime impacts are driven by a reduction in property crimes and serious traffic crimes that lead to a guilty court verdict. Violent and drug-related crimes remained unaffected. The reform also had a large impact on repeated criminal activity for men in the relevant cohorts. The crime rate of women, which was already much lower was not affected. Overall, the results confirm earlier findings of the impact of compulsory schooling reforms on crime in the US (Lochner and Moretti (2004)) and in the UK (Machin, Marie, and Vujić (2011)). The next chapter moves on to the impact on the child generation - which is a novel contribution in the literature. It also expands on the discussion of the results on crime in the parent generation.

## 1.8 Appendix - Chapter 1

### 1.8.1 Appendix - Descriptives

Table 1.8: Conviction Rates by Age and Type of Crimes Conditional on Being Convicted for Low SES Men in Parent Generation

	Percent convicted by crime types conditional on being convicted								
	Total	Violent	Property	Fraud	Traffic	Drugs	Sex	Others	Prison
<b>Panel A: Men of Parent Generation, low SES: at least one conviction</b>									
All	34.73	16.19	21.63	18.36	58.59	16.68	1.765	33.71	20.56
ages 20-24	20.32	11.28	26.40	12.22	44.32	16.85	0.408	31.07	17.23
ages 25-29	12.09	11.55	22.28	15.11	40.93	19.85	0.769	27.88	21.32
ages 30-39	13.27	14.94	19.57	19.68	44.97	15.06	1.362	28.87	22.44
ages 40-49	11.51	14.37	14.31	13.85	53.72	9.343	1.623	25.05	
<b>Panel B: Men of Parent Generation, low SES: multiple convictions</b>									
All	16.44	27.22	35.71	30.28	71.64	23.99	2.738	49.78	36.90
ages 20-24	7.143	20.00	48.98	24.66	54.49	24.28	0.693	47.97	36.80
ages 25-29	3.929	21.84	43.04	30.09	51.29	27.76	1.243	42.80	45.02
ages 30-39	4.653	27.50	38.52	34.47	57.46	22.95	2.029	43.87	45.57
ages 40-49	3.495	26.47	30.69	24.83	63.60	18.73	2.228	39.66	

Notes: Table shows overall conviction rates (Total) and type of crimes rates conditional on having been convicted at least once or twice. The sample are men with low SES, and for the age specific conviction rates only low SES men who are fully observed for the relevant age bracket are included. Panel A and B: All includes the sample low SES men born 45-55 (N=283,841) and the conviction rates refers to having ever been convicted or having been convicted at least twice between 1973-2010. Age specific conviction rates for men in Parent Generation includes: for ages 20-24 cohorts 53-55 (N=80,835), for ages 25-29 cohorts 48-55 (N=212,906), and for ages 30-39, and 40-49 the low SES sample of cohorts 45-55 (N=283,841).

Table 1.9: Conviction Rates by Age and Types of Crimes for Men Born 52-55

	Percent convicted by crime types conditional on being convicted								
	Total	Violent	Property	Fraud	Traffic	Drugs	Sex	Others	Prison
Panel A: Men born 52-55, all SES, Obs 176,232: at least one conviction									
All	38.62	15.94	24.25	18.19	58.49	18.73	1.465	35.36	19.44
ages 20-24	18.65	10.00	25.17	11.77	42.83	17.41	0.432	31.05	16.42
ages 25-29	11.61	11.31	22.87	16.22	38.24	21.35	0.660	28.97	21.32
ages 30-39	12.38	15.83	20.19	19.52	44.28	16.01	1.311	30.17	22.65
ages 40-49	11.30	14.27	13.13	12.50	56.54	9.996	1.486	24.85	
Panel B: Men born 52-55, all SES, Obs 176,232: multiple convictions									
All	18.88	26.63	38.94	29.72	71.11	27.90	2.278	51.59	34.66
ages 20-24	6.206	18.48	47.47	23.96	51.83	25.90	0.713	47.81	35.51
ages 25-29	3.824	20.92	43.97	31.38	47.97	31.35	0.920	43.78	44.84
ages 30-39	4.422	28.11	40.19	34.09	56.23	28.06	2.027	43.77	45.84
ages 40-49	3.528	25.24	28.95	21.97	64.95	21.83	1.914	40.16	
Panel C: Men born 52-55 low SES, Obs. 107,557: at least one conviction									
All	40.00	17.36	25.23	18.59	59.14	18.23	1.562	36.30	20.53
ages 20-24	19.79	11.05	25.89	12.20	44.11	16.39	0.456	31.04	17.12
ages 25-29	12.28	12.70	23.29	16.77	39.31	20.55	0.750	29.04	22.46
ages 30-39	13.19	16.92	21.09	19.49	45.00	15.12	1.417	30.42	23.67
ages 40-49	11.56	15.27	14.27	12.86	54.63	10.31	1.496	26.32	
Panel D: Men born 52-55, low SES, Obs. 107,557: multiple convictions									
All	20.07	28.61	40.10	30.16	71.73	26.66	2.413	52.60	35.95
ages 20-24	6.889	19.77	48.31	24.56	53.62	23.74	0.675	47.46	36.52
ages 25-29	4.192	23.38	44.82	31.96	49.41	29.94	1.042	43.65	46.66
ages 30-39	4.849	29.57	41.30	33.79	56.63	26.63	2.090	44.51	47.40
ages 40-49	3.734	26.54	30.55	22.29	63.99	22.24	1.718	41.46	

Notes: Table shows overall conviction rates (Total) and type of crimes rates conditional on having been convicted at least once or twice. The sample are all men of the Parent Generation born 52-55 with all SES (Panel A and B, N=176,232) or with low SES (Panel C and D, N=107,557).



Table 1.10: Conviction Rates by Age and Types of Crimes for Women of Parent Generation

	Percent convicted by crime types conditional on being convicted								
	Total	Violent	Property	Fraud	Traffic	Drugs	Sex	Others	Prison
<b>Panel A: Women of Parent Generation, all SES: at least one conviction</b>									
All	9.084	5.706	26.94	17.81	45.34	13.71	0.0594	15.77	5.766
ages 20-24	3.300	2.608	26.01	16.39	34.19	23.79	0	13.07	2.943
ages 25-29	2.272	3.151	25.61	19.23	32.13	20.10	0.0407	15.61	4.631
ages 30-39	3.070	5.060	28.36	20.32	39.84	10.80	0.0611	14.41	5.457
ages 40-49	2.623	6.469	27.69	15.25	45.40	6.970	0.0626	14.39	
<b>Panel B: Women of Parent Generation, low SES: at least one conviction</b>									
All	9.008	5.968	27.31	18.93	45.05	13.29	0.0576	15.52	6.149
ages 20-24	3.368	2.747	25.11	16.71	35.13	21.82	0	13.11	2.631
ages 25-29	2.229	3.567	25.55	20.47	32.86	19.39	0.0665	15.02	4.919
ages 30-39	3.118	5.256	28.70	21.72	40.12	10.40	0.0476	13.78	6.029
ages 40-49	2.628	6.674	29.19	16.27	43.35	6.914	0.0564	14.56	
<b>Panel C: Women born 52-55 all SES, Obs. 167,588: at least one conviction</b>									
All	10.22	5.996	27.42	18.80	45.37	15.99	0.0409	16.21	6.159
ages 20-24	3.210	2.603	25.77	16.38	33.87	23.72	0.0186	13.42	2.956
ages 25-29	2.359	3.542	26.21	22.54	31.93	19.78	0.0253	14.70	5.490
ages 30-39	2.936	6.016	30.55	21.48	41.91	10.30	0.0610	14.47	7.114
ages 40-49	2.604	7.333	26.81	15.03	45.14	9.120	0.0458	16.77	
<b>Panel D: Women born 52-55 low SES, Obs. 102,223: at least one conviction</b>									
All	10.21	6.350	27.65	19.85	45.10	15.38	0.0479	15.96	6.656
ages 20-24	3.259	2.852	24.95	16.69	34.97	22.16	0.0300	13.12	2.852
ages 25-29	2.300	3.828	26.16	23.99	31.82	19.44	0.0425	14.16	5.700
ages 30-39	3.022	6.280	30.79	23.28	41.73	9.938	0.0324	13.79	8.158
ages 40-49	2.592	7.623	29.02	15.32	42.72	9.585	0.0755	17.28	

Notes: Table shows overall conviction rates (Total) and type of crimes rates conditional on having been convicted at least once or twice for women in the Parent Generation, with all SES or low SES. Panel A and B: only women who are fully observed for the relevant age bracket are included. All includes the whole sample of women born 45-55 (all SES N=426,133, low SES N=269,701) and the conviction rates refers to having ever been convicted or having been convicted at least twice between 1973-2010. Age specific conviction rates for women in Parent Generation includes: for ages 20-24 cohorts 53-55 (all SES: N=126,625, low SES: 76,749), for ages 25-29 cohorts 48-55 (all SES: N=324,147, low SES: 202,430), and for ages 30-39, and 40-49 the whole sample of cohorts 45-55 (all SES: N=426,133, low SES: 269,701). Panel C and D: all conviction rates include only women born 52-55 (all SES: N=167,588, low SES: N=102,223).

## 1.8.2 Women and the Reform

Table 1.11: Reform Effects on Crime by Types of Crimes for Women of Parent Generation

	(1) Any	(2) Violent	(3) Property	(4) Drugs	(5) Traffic	(6) Fraud
Women born 52-55: All SES, Obs: 167,588						
Reform	0.389 (0.315)	0.039 (0.084)	-0.209 (0.214)	0.190 (0.135)	0.414* (0.238)	-0.120 (0.135)
$\bar{y}$ %	10.22	0.613	2.803	1.634	4.638	1.922

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between 1973-2010 for any crime in column (1), or one of the crime types specified in columns (2)-(6). Robust standard errors, clustered by municipality of birth, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of individual and father's education levels.

Table 1.12: Age Specific Reform Effects on Crime for Women of the Parent Generation

	(1) 30-39	(2) 25-29	(3) 20-24
Convicted at age:			
Women born 52-55, All SES, Obs. 167,588			
Reform	-0.013 (0.201)	-0.043 (0.166)	0.262 (0.194)
$\bar{y}$ %	2.936	2.359	3.210

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 30-39, 25-29, and 20-24. The sample are women from the parent generation born 52-55. Robust standard errors, clustered by birth municipality, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of the individual, and father's education levels.

### 1.8.3 Parent Estimations Including Trends

Table 1.13: Reform Effects on Years of Schooling for Parent Generation

Dependent variable: Own years of schooling			
	(1)	(2)	(3)
Sample:	All SES	Low SES	High SES
Reform	0.216*** (0.033)	0.309*** (0.034)	0.080** (0.036)
Obs	437,921	278,074	159,847
mean years	11.61	10.90	12.85

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. The dependent variable is number of years of own schooling. Robust standard errors, clustered by municipality of birth, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of individual and municipality group specific cohort trends. Column (1) includes father's education levels.

Table 1.14: Reform Effects on Crime by Types of Crimes for Parent Generation

	(1)	(2)	(3)	(4)	(5)	(6)
	Any	Violent	Property	Drugs	Traffic	Fraud
Panel A: Men born 52-55, All SES, Obs: 175,681						
Reform	-1.292 (0.921)	-0.525 (0.457)	-1.044* (0.558)	-0.943** (0.469)	-0.559 (0.793)	-1.139*** (0.383)
$\bar{y}$ %	38.62	6.160	9.364	7.223	22.58	7.022
Panel B: Men born 52-55, Low SES, Obs: 107,210						
Reform	-1.495 (1.053)	-0.825 (0.865)	-0.908 (0.648)	-1.309** (0.580)	-0.844 (0.950)	-1.269*** (0.470)
$\bar{y}$ %	40.00	6.949	10.10	7.282	23.65	7.433

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between 1973-2010 for any crime in column (1), or one of the crime types specified in columns (2)-(6). Robust standard errors, clustered by municipality of birth, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of individual, and municipality of birth group specific cohort trends. Panel A includes father's education levels.

Table 1.15: Age Specific Reform Effects on Crime for Parent Generation

	(1)	(2)	(3)
Convicted at age:	30-39	25-29	20-24
Panel A: Men born 52-55, All SES, Obs. 175,681			
Reform	-0.291 (0.573)	-1.261** (0.630)	-1.594** (0.711)
$\bar{y}$ %	12.37	11.61	18.65
Panel B: Men born 52-55, Low SES, Obs. 107,210			
Reform	-0.691 (0.695)	-1.403* (0.739)	-1.537* (0.849)
$\bar{y}$ %	13.18	12.27	19.79

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 30-39, 25-29, and 20-24. The sample are men from the parent generation born 52-55. Robust standard errors, clustered by birth municipality, in parentheses; all regressions include a full set of birth municipality and birth cohort indicator variables of the individual, and municipality of birth group specific cohort trends. Panel A also includes father's education levels.

Table 1.16: Reform Impact on Prison Sentences for Parent Generations

	(1)	(2)
Men born 52-55	All SES	Low SES
Reform	-0.495 (0.447)	-0.670 (0.527)
Obs	175,554	107,210
$\bar{y}$ %	7.505	8.206

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Effects scaled by 100. The dependent variable is an indicator variable for having ever been convicted for a prison sentence between 1973-2010. Robust standard errors in parentheses clustered by own birth municipality. Includes own birth cohort and birth municipality indicator variables, and municipality of birth specific cohort trends. Column (1) includes father's education levels.

## Chapter 2

# Intergenerational Effects of Education Policy on Crime

### 2.1 Introduction

A strong intergenerational correlation in criminality and the fact that education policies can affect parental resources as well as skills important for parenting suggests that education policies could also have intergenerational effects on crime. So far, the question if education policies can have such intergenerational effects remains open. There are good reasons to expect so considering, the intergenerational associations of criminal behavior documented in the criminology literature. Also, for Sweden Hjalmarsson and Lindquist (2012) document a strong correlation between crime of fathers and children of both genders using the Stockholm Birth Cohort Study. A second Swedish study by Hjalmarsson and Lindquist (2013) uses adoption data to determine factors through which parents affect criminal behaviour of their children. They document a strong parent-child correlation in criminal behaviour.

In general, child outcomes will be driven by predetermined parental characteristics and by the investments parents (and possibly the state) undertake to promote the child's human capital (see Becker (1981)).<sup>1</sup> Cunha, Heckman, and Schennach (2010) formalize the intergenerational links and show the importance of parental background and investments for child cognitive and social skill outcomes. An implication of their results is that improving parental skills will have a direct impact on their children,

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<sup>1</sup>For some evidence on the importance of mother's education on child outcomes see for example Carneiro, Meghir, and Parey (2013); Deming (forthcoming) highlights the importance of school quality and its potential impact on crime.

while the increased parental resources may increase investments leading to further intergenerational improvements (see also Cunha (2007) and Caucutt and Lochner (2012)).

Several theories originating from sociology and social behavior also predict a causal relationship between family resources and criminal behavior of the offspring. Merton (1938) suggests that lack of means to fulfill culturally defined societal goals may cause some individuals to reject legitimate means of achieving these goals. Coleman (1988) stresses the importance of interaction between parental human capital and other family resources - such as parental attention, control and quality of parent-child relations - in the formation of child human capital. There is direct evidence that better childhood environments and early education can reduce crime rates as shown by the Perry pre-school experiment presented in Schweinhart, Montie, Xiang, Barnett, Belfield, and Nores (2005) and Cunha and Heckman (2007).

The focus in this chapter is estimating the impact of educational interventions received by the parents on child crime outcomes. Specifically, we show that the Swedish comprehensive school reform, that we showed substantially decreased crime rates of the target generation, also decreased crime of their children. The reason we may expect this intergenerational effect is because men affected by the reform attain higher education levels, have improved cognitive and social skills, earn substantially more, engage less in criminal activity, and marry higher earning wives.

## 2.2 Data and Descriptives for Child Generation

The data used for the empirical analysis of the reform effects on the children of those directly affected by the reform comes from the same data source. Again, we link three generations in the data: *parent*, *grandparent* and *children generation*. But, in this chapter the focus lies on crime of the *children generation*. To reiterate, the children generation is defined as the sons of men (or women) in the parent generation who were born between 1945 and 1955. For each son we determine father's (mother's) reform assignment based on father's (mother's) municipality of birth. The details of the reform and how we assign reform status to individuals are presented and discussed in detail in the previous chapter, in Section 1.2. Conviction data for the children generation is covering the time period between 1973 and 2010 and has been linked to sons in our data set using the unique personal identifying number. Exploiting the

details of the crime data we are able to identify seven types of crime categories and the age when it was committed, and details of the crime data outlined in chapter 1 apply likewise. We define the main age observation window for sons' convictions between the ages 15 to 29. We select the sample of sons of men born 1945-1955, who have reached the age of criminal responsibility (age 15) in 2008. Furthermore, sons have to be alive in 1973 (when the crime records begin) and have not migrated permanently out of Sweden. For this sample of sons we are able to match education information of paternal grandfathers for 61.53 percent from the Swedish National Education Register. For estimations regarding mother's reform assignment, we select sons of mothers who were born between 1945 and 1955.

Similarly, to Chapter 1 we first report conviction rates for sons in the child generation. Under the column "Total", Table 2.1 shows the overall conviction rate for sons of men born 45-55. Again, we also report the conviction rate by type of crime *conditional* on being convicted in the remaining columns. Since individuals can have multiple convictions these columns add up to more than 100. The conviction rates correspond to the observation period 1973-2010 over the 15-29 age range.

Table 2.1: Conviction Rates by Age and Types of Crimes for All SES

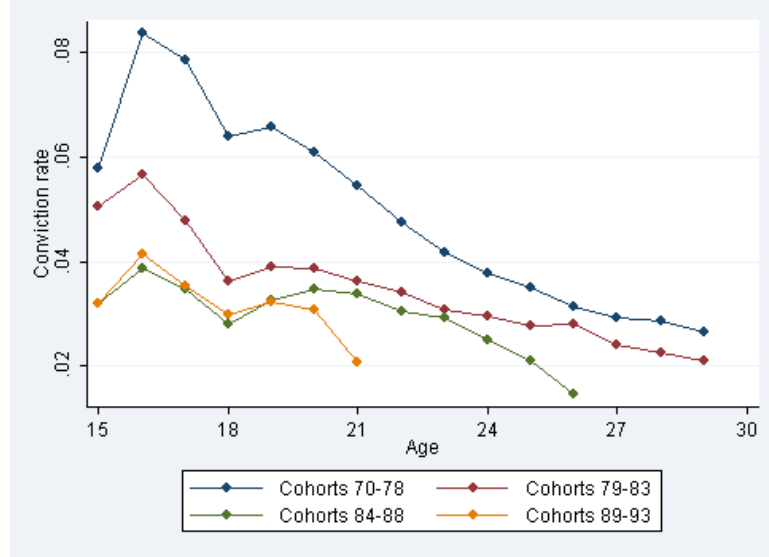
	Percent convicted by crime types conditional on being convicted								
	Total	Violent	Property	Fraud	Traffic	Drugs	Sex	Others	Prison
<b>Panel A: Sons in Child Generation: at least one conviction</b>									
All 15-29	23.69	19.23	33.07	12.28	46.03	13.63	0.881	31.18	10.26
ages 15-19	15.83	15.95	39.81	11.40	35.51	7.527	0.518	24.68	2.726
ages 20-24	11.80	17.71	20.59	8.303	41.44	15.48	0.736	30.02	15.30
ages 25-29	7.948	16.27	14.29	8.627	48.89	18.88	0.978	24.73	17.17
<b>Panel B: Sons in Child Generation: multiple convictions</b>									
All 15-29	7.456	36.48	57.48	23.06	58.86	27.26	1.264	50.55	24.46
ages 15-19	5.107	30.04	62.42	18.65	45.55	12.84	0.776	39.42	6.943
ages 20-24	3.636	29.90	40.59	16.63	49.04	30.55	0.825	43.73	33.75
ages 25-29	2.387	25	32.70	16.98	56.78	38.89	1.109	37.28	37.98

Notes: Table shows overall conviction rates (Total) and type of crimes rates conditional on having been convicted at least once or twice. The sample are men with all SES. For the age specific conviction rates only men who are fully observed for the relevant age bracket are included. For Panel A and B: All 15-29 includes the whole sample of sons born in or before 1993 (N=426,721), and the conviction rates refers to having ever been convicted or having been convicted at least twice between the age 15-29. Sons' conviction rates by age includes: for ages 15-19 the whole sample of sons (N=426,721), for ages 20-24 cohorts born in or before 1988 (N=380,249), and for ages 25-29 cohorts born in or before 1983 (N=294,749).

About 24 percent of sons in the child generation had a conviction in our observation age window of 15-29. Conditional on a conviction there are substantial numbers convicted of violent and property crimes as well as traffic crimes. The Table also reports conviction rates by age. Comparing the crime rates at the same ages with that of the parent generation in Table 1.9 we see a very large decline in crime across the generations. For both generations most offenses are committed by the younger

individuals. Figure 2.1 shows crime age profiles for sons. As for the parent generation, sons crime age profiles show a pronounced decline of crime with age. Interestingly, there are very strong cohort effects on crime for the child generation, showing a marked decline over time.

Figure 2.1: Age Profile of Crime Rate for the Child Generation by Cohort



Column (7) and the bottom Panel of Table 2.1 provides information on more serious criminal activity, namely convictions leading to prison and multiple (2+) convictions, as well as the percent convicted of each type of crime conditional on having 2+ convictions. The distribution by type of crime involves more violent, property and fraud crime, although traffic related crimes are also prevalent. About 10 percent of those convicted in the child generation were incarcerated (over the 15-29 age range). Moreover, when we consider multiple convictions the prevalence of violent, fraud and property crimes increases.

### 2.2.1 Parental Background, Education and Crime

We now move on to document the intergenerational correlation between parental education and crime, and father's crime and son's crime. Table 2.2 shows the results from regressing conviction (columns 1 and 3) and incarceration (columns 2 and 4) on father's and mother's education based on a Linear Probability Model. All regressions include dummies for the municipality of birth of the father and cohort effects.

One year of own schooling for men in the parent generation is associated with a decrease of the probability of a conviction by 2.5 percentage points corresponding to a 7.4 percent reduction in conviction rates. For the child generation (and including



Table 2.2: Association Between Own and Parental Education and Crime

	(1)	(2)	(3)	(4)
	Men born 45-55		Sons of parents born 45-55	
	Conviction	Prison	Conviction	Prison
Own Schooling	-2.496*** (0.114)	-0.998*** (0.059)	-4.246*** (0.036)	-0.754*** (0.020)
Father's Schooling			-0.218*** (0.033)	-0.038*** (0.012)
Mother's Schooling			-0.087** (0.040)	-0.029*** (0.010)
Obs	444,272	444,272	273,093	273,093
$\bar{y}$ %	33.88	6.597	24.48	2.371

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Effects scaled by 100. Dependent variables for men born 45-55: indicator variables for having ever been convicted or having received a prison sentence between 1973-2010. Dependent variables for sons: indicator variables for having ever been convicted or having received a prison sentence between the ages 15-29. Robust standard errors in parentheses clustered by own birth municipality or by father's birth municipality. Includes own birth cohort and birth municipality indicator variables, or father's cohort and father's birth municipality indicator variables.

parental education) the coefficient on own education increases substantially for conviction, but less for incarceration. Both father's and mother's education are significant but the impact of the former is larger.

Table 2.3: Association Between Son's and Father's Crime

	(1)	(2)	(3)	(4)
	All SES		Low SES	
	Convict	Prison	Convict	Prison
Panel A: Fathers born 45-55 and their sons born before 1994				
Father convict/prison	12.503*** (0.187)	6.205*** (0.168)	12.895*** (0.215)	6.699*** (0.235)
Obs	410,475	410,475	261,918	261,918
$\bar{y}$ %	23.54	2.380	25.09	2.682
Panel B: Fathers born 53-55 (ages 20-29), sons born before 1982 (ages 20-29)				
Father convict/prison at ages 20-29	12.000*** (0.627)	9.073*** (0.772)	12.457*** (0.635)	10.086*** (0.998)
Obs	37,006	37,006	24,956	24,956
$\bar{y}$ %	18.42	3.437	18.94	3.614

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Effects scaled by 100. The dependent variables are indicator variables for sons having been convicted or sentenced to prison between the ages 15-29 in Panel A, and between the ages 20-29 in Panel B. Robust standard errors in parentheses clustered by father's birth municipality. Includes father's cohort, sons' cohort and father's birth municipality indicator variables.

Finally, Table 2.3 illustrates the intergenerational associations of crime. The probability of ever being convicted increases by over 12 percentage points if the father has been convicted. The father having been jailed is associated with a 6 percentage point increase in the probability that the son will go to prison too. These associations do not change much when we take just low SES individuals.

In interpreting these results there is an issue with the alignment of ages across generations as pointed out by Haider and Solon (2006) in the context of intergenerational mobility of income. So in Panel B we present the intergenerational association for ages 20-29 for both generations, controlling for cohort effects for both. For the conviction rates the intergenerational associations are essentially the same when we align ages in this way. However, for prison they increase by 50 percent. Though, the inescapable conclusion from these results is that there is a very strong intergenerational association of crime and incarceration.

## 2.3 Empirical Strategy

The main outcome variables in this chapter for the children generation are whether a son was ever convicted during the observation window 1973-2010 over the 15-29 age range for any crime and by type of crime; and whether a son was convicted at certain ages 15-19, 20-24 and 25-29. Again, we present results for the whole sample and separately for those with a low SES background.

For the child generation we observe the criminal history from the age 15, when criminal responsibility begins and crimes are recorded according to Swedish law. We follow them until the age of 29. This allows us to measure the effects on the most important part of the criminal lifecycle. The child generation all attend the same schooling system because the reform had been rolled out nationally at that point. The only difference is that for some the fathers also attended the new system, while for others they did not. The children of both treated and untreated fathers live in the same labor market areas and their fathers belong to all the cohorts 1945-55 of the transitional period. We also explore the impact of exposing females to the reform on the crime rate of their sons.

As mentioned in the previous chapter the reform was not randomized. We control for potential differences across treatment and control municipalities using a difference in differences approach, just as we did for the parent generation. This section will highlight the estimation strategy followed for the children generation. We compare the change in son's crime across father's cohorts in municipalities that implemented the reform for the younger cohort of father's but not the older one to the change in crime rate across the same cohorts of father's living in municipalities where there was no change in policy for these same cohorts. In practice we do this for all 11

father's cohorts in our window and all 1000 or so father's birth municipalities. Thus our approach is best described by the regression,

$$(2.1) \quad \tilde{y}_{i,m,t}^c = \alpha + \beta_1 R_{i,m,t}^f + \gamma_1' t_i^f + \gamma_2' M_i^f + \epsilon_{i,m,t},$$

where  $\tilde{y}_{i,m,t}^c$  is the latent child crime "intensity" outcome observed for son  $i$  in the child generation whose father was born in municipality  $m$  and in birth cohort  $t$ . A conviction corresponds to  $\tilde{y}_{i,m,t}^c > 0$ .  $R_{i,m,t}^f$  is the reform indicator, which equals one if the father of son  $i$  belongs to a municipality and cohort that has been assigned to the new school system;  $t_i^f$  is a vector of indicator variables indicating to which cohort the father of son  $i$  belongs to and  $M_i^f$  is a vector of indicator variables indicating in which municipality the father of son  $i$  was born.  $\epsilon_{i,m,t}$  is conditionally independent of  $R_{i,m,t}^f$ . Both the general assumptions underlying the method as discussed in Chapter 1, and the reasoning for the chosen estimation procedure as explained in Chapter 1 apply.

## 2.4 The Reform and Crime in the Child Generation

The reform can only have an effect through the parents having been affected because at this time all children were attending the new school system. For these estimations we take all the sons of fathers born between 1945-55, some of whom will have been exposed to the reform and others not. This is a broader group than the one we used to estimate the impacts on the parent generation: while we do not observe the older parents cohorts at a young enough age to estimate impacts of the reform on their crime rates we can certainly use their exposure to the reform to measure the impact on children.

In Table 2.4 we show the impact of the reform on the probability of conviction in the child generation for any age between the ages of 15-29 inclusive. The first column shows the results for the entire sample and columns 2 through 4 show the effect in different age groups, separately. Panel B in the table shows the results for those whose fathers were born in low SES homes.

The overall result is a highly significant reduction in criminality of 0.78 percentage points (pp) in the child generation. The point estimate is similar, and also significant,

Table 2.4: Age Specific Reform Effects on Son's Crime

	(1)	(2)	(3)	(3)
Convicted at age:	15-29	15-19	20-24	25-29
Cohorts observed:	1960-1993	1960-1993	1960-1988	1960-1983
Panel A: All SES				
Reform father	-0.779*** (0.257)	-0.589*** (0.210)	-0.314 (0.203)	-0.107 (0.186)
Obs	410,476	410,476	365,782	283,297
$\bar{y}$ %	23.54	15.70	11.69	7.861
Panel B: Low SES				
Reform father	-0.667** (0.326)	-0.567** (0.267)	-0.196 (0.249)	0.210 (0.238)
Obs	261,918	261,918	236,289	187,515
$\bar{y}$ %	25.09	16.81	12.44	8.255

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 15-29, 15-19, 20-24 or 25-29. The sample are sons of men born 45-55 who are fully observed for the relevant age bracket of the dependent variable. Robust standard errors, clustered by father's municipality of birth, in parentheses; all regressions include a full set of father's birth municipality and father's birth cohort indicator variables. Panel A includes grandfather's education levels.

in the group originating from low SES families. The division of the sample by age groups shows that the effect is largest for the younger (15-19) age group and declines for older groups.

Table 2.5: Reform Effects on Son's Crime by Type of Crimes

	(1)	(2)	(3)	(4)	(5)
Convicted at age 15-29:	Violent	Property	Drugs	Traffic	Fraud
Panel A: All SES, Obs 410,476					
Reform father	-0.243** (0.121)	-0.019 (0.158)	0.095 (0.110)	-0.446** (0.178)	-0.224** (0.097)
$\bar{y}$ %	4.485	7.736	3.182	10.82	2.877
Panel B: Low SES, Obs 261,918					
Reform father	-0.108 (0.163)	0.022 (0.203)	0.081 (0.139)	-0.444* (0.238)	-0.233* (0.120)
$\bar{y}$ %	4.946	8.406	3.199	11.86	3.109

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 15-29 for one of the specified crime types. Robust standard errors, clustered by father's municipality of birth, in parentheses; all regressions include a full set of father's birth municipality and father's birth cohort indicator variables. Panel A includes grandfather's education levels.

Table 2.5 splits up the effect by type of crime. Such analysis is important because different types of crime have a different social cost and may have different underlying motivations, which in turn is suggestive about the way the reform affected crime outcomes. We see that the effects that dominate are the reduction of violent crime, traffic and fraud each by about 0.24-0.45pp. Interestingly, property and drugs crime

seem unaffected with the estimates being effectively zero. Focusing on the low SES group does not change these conclusions.

Table 2.6: Reform Effects on Son's Recidivism and Incarceration

Dependent variables:	Multiple convictions/Recidivism				Incarceration
	(1)	(2)	(3)	(4)	(5)
Convicted at age:	15-29	15-19	20-24	25-29	15-29
Cohorts observed:	1960-1993	1960-1993	1960-1988	1960-1983	1960-1993
Panel A: All SES					
Reform father	-0.267 (0.172)	-0.183 (0.129)	-0.134 (0.120)	-0.154 (0.108)	-0.087 (0.092)
Obs	410,476	410,476	365,782	283,297	410,286
$\bar{y}$ %	7.343	5.033	3.570	2.337	2.380
Panel B: Low SES					
Reform father	-0.166 (0.225)	-0.186 (0.172)	-0.056 (0.148)	0.001 (0.163)	0.009 (0.124)
Obs	261,918	261,918	236,289	187,515	261,918
$\bar{y}$ %	8.077	5.542	3.924	2.505	2.682

Notes: Significance levels \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results are scaled by 100. The dependent variables are indicator variables for having been convicted at least twice between the ages of 15-29, 15-19, 20-24 or 25-29, or an indicator variable for having ever been convicted for a prison sentence between the ages 15-29. The sample are sons of men born 45-55 who are fully observed for the relevant age bracket of the dependent variable. Robust standard errors, clustered by father's municipality of birth, in parentheses; all regressions include a full set of father's birth municipality and father's birth cohort indicator variables. Panel A includes grandfather's education levels.

The reform also increased schooling for women, albeit a bit less. However, as we show in Appendix Tables 2.10 and 2.11 there is no effect of exposing the mother to the reform on the criminal activity of male children.

We now turn to the impacts on recidivism and incarceration. For recidivism, Table 2.6 shows the effects are small and overall they are significant only at the 12 percent level. For the child generation the incarceration rate for the 15-29 age group is 2.4 percent over the age window we observe. We again experimented with a probit using the fixed effects based on the first father cohort for which the reform was implemented in the municipality; as for the parents this probit for sons gave identical results to the LPM, which we report here. The results are presented in the last column of Table 2.6 and show that the coefficients are small and not significantly different from zero, although they are quite precisely estimated. Both recidivism and incarceration are very low in the child generation and perhaps it is not surprising that we do not observe any effects, since these may be really the hardened offenders.

## 2.5 Common Trends Assumption

One of the key identifying assumptions of our approach is that the underlying trends in crime are the same irrespective of the birth municipality of the parent generation. We now bring to bear evidence for this assumption in three different ways: first, we repeat our estimation assuming that the reform took place at a different date than it actually did (placebo estimations); second, we explicitly include municipality specific trends to evaluate whether they are significant (akin to a test of overidentifying restrictions) and whether our results are sensitive to their inclusion; third, we plot residuals to show that they do not display a trend. For these tests we group municipalities by the earliest cohort for which they implemented the reform and we look for omitted trends specific to each of these groups.

For the placebo estimations, where we pretend that the reform was implemented later, we only use the sample of sons whose fathers were treated by the reform. To construct placebo treatment and control groups we then pretend that the reform was implemented successively one year later, two years, three years, etc. We (falsely) assign the first treated cohort (the first two treated cohorts, the first three treated cohorts, etc.) in each municipality group to be untreated and the remaining ones to the treated group. This provides five placebo estimates.

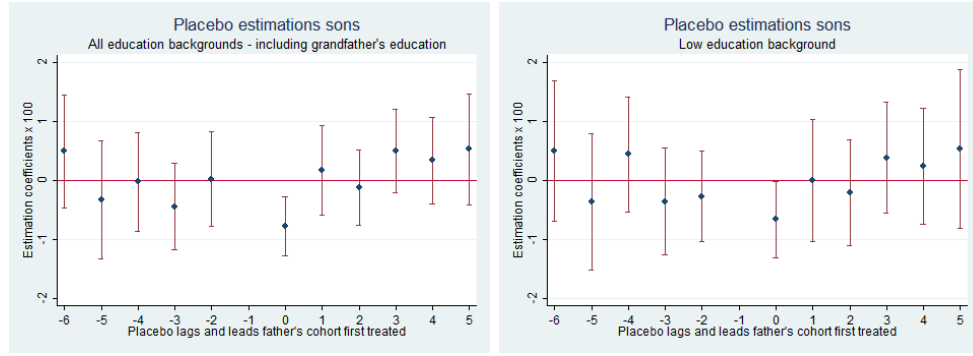
Similarly, for the placebo estimations where we pretend that the reform was implemented earlier, we restrict the sample to sons whose fathers were not treated by the reform.<sup>2</sup> The placebo treatment groups are defined by (falsely) assigning the two last untreated cohorts (the three last untreated cohorts, the four last untreated cohorts, etc.) to the treated group and the remaining cohorts stay in the control group. This provides an additional five placebo estimates.

The results are all brought together in Figure 2.2. Each dot represents the estimate assuming the reform took place at the specified period on the x-axis (relative to when it actually took place, which is the zero point). The vertical line around the dot represents the 95 percent confidence interval. The graph shows that the largest (in absolute value) and only significant effect is obtained when we use the correct timing for the reform assignment (that is at zero). In all other cases we estimate insignificant effects and no particular pattern shows up implying there is nothing systematic taking

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<sup>2</sup>We require at least two treated cohorts and one untreated cohort in each municipality group to implement the estimator. This means that we start our first placebo estimation pretending the reform was implemented two years earlier than it actually was.

Figure 2.2: Placebo Estimations Sons



place biasing the results towards an effect on crime.

For our second approach, Table 2.7 shows F-statistics and p-values for two tests: that the trends are not specific to groups of municipalities (sorted by cohort of first implementation) and that the impacts remain unaffected by the inclusion of such trends. For the child generation we find no evidence that such specific trends are present for the overall sample. While, the trends for the child generation of those from lower SES backgrounds are however marginally significant (p-value 4.4 percent). Nevertheless, as is clear from columns (3) and (4) in this table, this marginal significance does not translate to a significant effect on the impacts. For example, the overall effect without any municipality specific trends is -0.779 (se 0.257) while when we include trends this becomes -0.800 (se 0.276). For the lower SES group the effect drops a bit from -0.667 (se 0.326) to -0.571 (se 0.356). The parameter estimates obtained when these trends are included are shown in the Appendix in Table 2.12. None of these changes are significant. There is no change in terms of precision for the child generation, which is in contrast to the loss in precision for the parent generation when including trends. This is probably because we cover the children of all relevant parental cohorts (1945-55) and the sample size is much larger than in our parent sample.

Table 2.7: Trends Tests for Child Generation

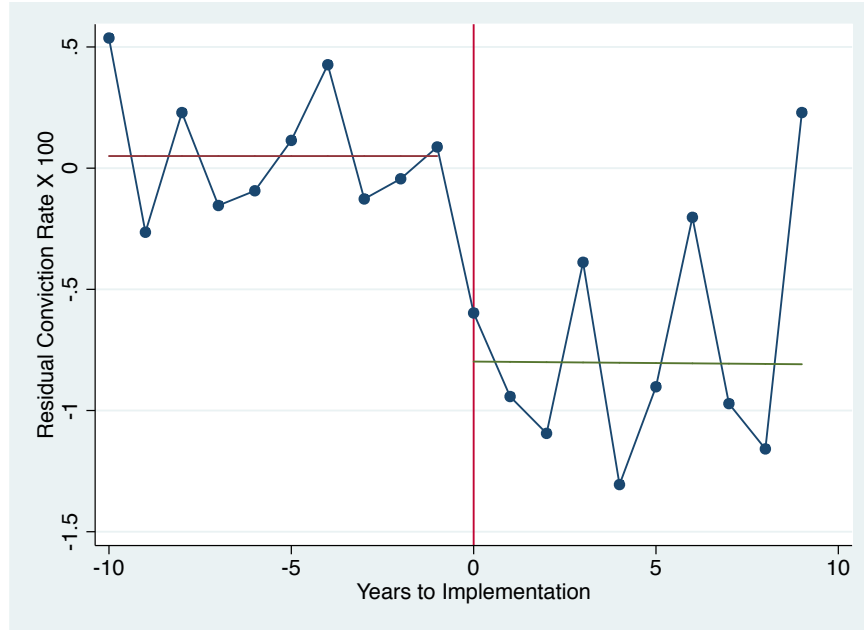
Tests:	Joint test of trends=0		Reform parameter across models	
	(1)	(2)	(3)	(4)
	All SES	Low SES	All SES	Low SES
F/chi2 statistic	1.235	1.665	0.0338	0.320
P-value	0.230	0.0436	0.854	0.571

Notes: Test 1 jointly tests the hypothesis that trends are common across municipalities. Test 2 tests the hypothesis that the impacts are the same when comparing the specification with and without trends.

Turning now to a graphical representation, in Figure 2.3 we plot the residuals from

sons' difference-in-differences regressions (with the estimated average impact on sons crime added back in) grouped by years to implementation. Each point corresponds to an average residual across fathers cohorts in different municipalities grouped by their years to implementation.<sup>3</sup> If there are systematic trends in sons crime related to early or late implementing municipalities these would show up as a trend in these residuals because the composition of municipalities changes as we move along the x-axis to different times to implementation. However, these residuals display no trend: the pre-implementation trend is -0.0025 and the post implementation one is zero to 4 decimal points. This completes what we view as conclusive evidence that the results we present on the intergenerational impacts of the reform are robust and not a spurious artifact of other events in the data.

Figure 2.3: Trend Graphs Child Generation



### 2.5.1 Municipality Characteristics and Exogeneity of the Reform

The fixed effects approach controls for permanent confounding differences in the municipalities that implemented the reform for different cohorts. However, if there are important time varying characteristics that are correlated with crime rates this could

<sup>3</sup>For example if municipality 1 implemented the reform for the 1948 cohort, this cohort would contribute to the zero point on the graph, the 1947 cohort contributes to -1 and so on. Going forward 1949 would contribute to +1, 1950 to +2 etc. This is repeated for all municipalities by time to implementation. The residuals are then averaged by this time to implementation because presenting these trends one by one is too noisy to be visually informative.



lead to biases. In our empirical work we already control for one such variable namely the education level of the grandparent generation which can be correlated with the crime rates of the child generation (or the parent generation in Chapter 1). Indeed this variable is significant and when we include it the overall estimated impact of the reform on crime increases (from -0.5 to -0.78). The various tests of differential trends should have power against other omitted time varying characteristics. The fact that these tests have supported the robustness of our results corroborates our assumption that the reform can be taken to be conditionally exogenous.

### 2.5.2 Discussion

The Swedish educational reform reduced the crime rates of men of both the direct subjects of the reform (the parent generation) as well as that of their sons. For the parent generation the impacts are driven by a reduction in property crimes and those traffic crimes serious enough to lead to a court appearance. Violent and drug-related crimes remained unaffected. The reform also had a large impact on repeated crime for men. The crime rate of women, which was already much lower was not affected.

For the child generation the impacts are driven by declines in fraud (including tax evasion) as well as violent crime and traffic offenses. Thus, the impact relates both to crimes with a clear economic motivation (fraud) as well as to crimes relating more to anti-social behavior (violent, traffic). These results relate to the case where the father was exposed to the reform. When instead we consider the impact of treating the mother we find no impact on the child generation, despite the fact that the years of education increased for women as well.

The persistence of the effects of this educational policy across generations puts a different perspective on the value of such reforms. However, understanding the mechanisms through which the reform achieved these effects is complicated by the multiple possible channels. In Table 2.8 we present impacts on a number of outcomes as information to help understand the channels that operated. We do not, however, claim to offer conclusive evidence on mechanisms. After all we only have one discrete source of variation.

For the men of the parent generation, who were the direct subjects of the reform, theory points to the improved economic opportunities in the legal labor market resulting from increased education as a key factor leading to a reduction in crime

participation (see e.g. Becker (1981), Freeman (1999)). In fact, human capital and economic opportunity did improve as a result of the reform: educational attainment increased and as reported in Meghir, Palme, and Simeonova (2013) the reform led to a 0.12 of a standard deviation (se 0.044) increase in cognitive skills for those with low education fathers. Moreover, as shown in Table 2.8, the reform translated to higher earnings for the parent generation.<sup>4</sup> This in itself increases the opportunity cost of crime. Meghir, Palme, and Simeonova (2013) also report an increase in the armed forces social skills indicator of 17 percent of a standard deviation (se 0.077) as a result of this reform;<sup>5</sup> interestingly this increase in social skills is driven mainly by those from a higher SES background for whom the social skills indicator increased by 0.53 of a standard deviation (se 0.198). This demonstrates that the reform affected all groups. We know from Heckman, Stixrud, and Urzua (2006) that an improvement in cognitive and social skills reduces antisocial and risky behavior and improves earnings. The reduction in criminality we observe here is consistent with this.

A decline in crime and an improvement in the cognitive and social skills in the parent generation can induce directly a reduction in crime in the child generation through better parenting practices. Moreover, there was an increase in parental resources both because fathers' earnings increased and because fathers exposed to the reform had children with women who earn more (about \$820 per annum measured in 2004) and who are marginally less likely to be unemployed.<sup>6</sup> Taken together and assuming that child investments are driven by life-time income these results imply an increase in resources available for child investments. Moreover, fertility does not increase and hence these increased resources do not get diluted. Having children as a teen also declined but from a very low base: the table shows a 0.263 percentage point decline from a baseline of 1.7 percent of fathers having a child while being teens.

Taken together, the evidence points towards increased resources at the home and improved parental quality, which should lead to better upbringing for the children. This is consistent with reductions in criminal activity of the child generation.

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<sup>4</sup>This is consistent with Meghir and Palme (2005).

<sup>5</sup>The test is administered to army conscripts. Military service was compulsory in Sweden at that time

<sup>6</sup>For this impact we use levels since some women have zero earnings. We do not condition on whether the couple is married or not; we just use information on who is the mother of the child. It is interesting to note that we have not found any direct evidence that female earnings increased as a result of the reform. So this result indicates an improvement in matches for men treated by the reform and not just a mechanical effect that men are having children with younger women in the locality and who are treated as a result.

Table 2.8: Impact of the Reform on Further Outcome Variables in Both Generations

	(1) All SES	(2) Low SES
Panel A: Men born 45-55		
Log annual earnings $\times 100$	6.4** (3.0)	6.9*** (2.4)
Ever had a child $\times 100$	-0.093 (0.185)	-0.096 (0.273)
Number of children	-0.004 (0.007)	0.001 (0.010)
Age at birth first child	0.106 (0.075)	0.064 (0.048)
Child born while a teen $\times 100$	-0.263** (0.106)	-0.210** (0.100)
Spouse education	0.0499 (0.061)	0.0274 (0.0274)
Spouse annual earnings in SEK	5,462** (2,672)	4,829 (3,361)
Spouse unemployed	-0.003*** (0.001)	-0.0006 (0.001)
Panel B: Sons of men born 45-55		
Years of schooling (measured at age 25)	-0.002 (0.017)	-0.0001 (0.021)
Disposable income (measured at age 25) $\times 100$	0.084 (0.401)	-0.472 (0.503)

Notes: Significance levels \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Each row corresponds to a separate specification, estimating the reform impacts on the dependent variables specified. We use the level of spousal earnings instead of the log to accommodate zeros when the spouse is not working. Disposable income is defined as the sum of incomes from capital, labor, income security programs and allowances minus all income taxes. Robust standard errors clustered at the birth municipality of individual (Panel A) or birth municipality of father (Panel B). All estimations include birth cohort and birth municipality indicator variables (Panel A), or father's birth cohort and father's birth municipality indicator variables (Panel B).

A possible puzzle in these results is that the reform does not lead to improvements in other outcomes in the child generation: as can be seen in Table 2.8, the children of those who went through the reform did not attain higher levels of education relative to those with untreated fathers. Moreover, the effect on earnings at 25 is also zero. The educational result is in general confirmed by those obtained by Holmlund, Lindhal, and Plug (2011).<sup>7</sup>

In interpreting these results it is important to note that low ability individuals are

<sup>7</sup>Lundborg, Nilsson, and Rooth (2012) use an IV strategy to estimate the effect of years of schooling on a number of outcomes for the child generation. They do find a number of significant effects, particularly of mother's schooling. However, their results depend on the validity of using the reform as an exclusion restriction. They do not report the reduced form effect of the reform other than on schooling.

likely to be constrained by the compulsory schooling laws and may not be willing to invest one whole extra year of education. Indeed, when we estimated a simple quantile regression for the 25th percentile of schooling we found that the intergenerational persistence between the child and the parent generation was significantly higher when the father had gone through the reform. This reflects the fact that the children were constrained by the compulsory schooling laws. At the 75th percentile there was no effect of the reform on intergenerational persistence. Still we could observe an effect on earnings. However, the negotiated minimum wages prevalent in Sweden may be masking any subtle improvements at the bottom of the distribution.<sup>8</sup>

Thus, despite the lack of effects in these other dimensions, human capital may have increased sufficiently at the lower part of the distribution to induce a reduction in crime. In addition, the propensity to commit crimes does not only depend on human capital, but also relates broadly to the psychic costs of crime, such as moral values and other personal preferences, including attitudes to risk. These characteristics are hard to measure directly, but are likely to be affected by home environments and parental resources.

## 2.6 Conclusions

This chapter considers the intergenerational effects of educational policy on crime. In Chapter 1, we first confirmed that a Swedish educational reform of the 1950s which increased compulsory schooling and abolished tracking reduced crime substantially. This is consistent with results found for the US and the UK.

The new question addressed in this chapter is whether exposing fathers to the reform has an impact on the crime rates of the next generation, given that all the children are in any case educated under the new reformed system. The reason we may expect this to happen is because of improved parental human capital, which may translate to better parenting and greater availability of resources as indeed is the case.

Our results establish substantial impacts of father's exposure to the reform on the child generation crime rates: it resulted in an overall decline in the crime rate by about 0.8 percentage points, mostly driven by a decline in convictions among the 15-19 year olds. The reductions are mainly concentrated among violent crime, traffic

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<sup>8</sup>We do not have at our disposal measures of cognition for this cohort yet although we hope to obtain them.

crime (serious enough to lead to a court case) and fraud - including tax evasion. We are not able to conclusively establish the mechanisms that led to such a reduction. We are, however, able to establish that home environments for children in families where the father was exposed to the reform improved in a number of dimensions. That these improvements led to a reduction in criminality of their children is consistent with both theories of intergenerational transmission of human capital (see e.g. Becker and Tomes, 1979, or Cunha and Heckman, 2007) as well as sociological theories on the effect of strains (see Merton (1938)) and formation of social capital (see Coleman (1988)).

## 2.7 Appendix - Chapter 2

### 2.7.1 Additional Descriptive Statistics - Children

Table 2.9: Conviction Rates by Age and Type of Crimes Conditional on Being Convicted for Low SES Sons

	Percent convicted by crime types conditional on being convicted								
	Total	Violent	Property	Fraud	Traffic	Drugs	Sex	Others	Prison
<b>Panel A: Sons of Child Generation, low SES: at least one conviction</b>									
All 15-29	25.23	19.90	33.74	12.48	47.32	12.91	0.918	31.37	10.86
ages 15-19	16.95	16.29	40.40	11.34	36.90	6.993	0.562	24.34	2.907
ages 20-24	12.54	18.31	21.22	8.607	42.05	14.50	0.747	30.26	16.15
ages 25-29	8.344	17.06	14.96	8.875	48.42	18.33	0.996	25.27	17.58
<b>Panel B: Sons of Child Generation, low SES: multiple convictions</b>									
All 15-29	8.203	37.16	58.13	23.57	60.01	25.99	1.358	50.63	25.20
ages 15-19	5.625	30.21	63.11	18.84	46.85	12.18	0.837	38.90	7.275
ages 20-24	4.002	30.29	41.21	17.11	49.69	28.72	0.937	43.94	34.26
ages 25-29	2.556	25.96	33.76	17.63	56.35	38.03	1.145	37.87	38.67

Notes: Table shows overall conviction rates (Total) and type of crimes rates conditional on having been convicted at least once or twice. The sample are men with low SES, and for the age specific conviction rates only low SES men who are fully observed for the relevant age bracket are included. For Panel A and B: All 15-29 includes the low SES sample of sons born in or before 1993 (N=271,971), and the conviction rates refers to having ever been convicted or having been convicted at least twice between the age 15-29. Sons' conviction rates by age includes low SES sons: for ages 15-19 the whole low SES sample of sons (N=271,971), for ages 20-24 cohorts born in or before 1988 (N=245,342), and for ages 25-29 cohorts born in or before 1983 (N=194,854).

### 2.7.2 Mother's Reform Effects on Crime

Table 2.10: Age Specific Reform Effects of Mothers on Son's Crime

	(1)	(2)	(3)	(4)
Convicted at age:	15-29	15-19	20-24	25-29
Cohorts observed:	1960-1993	1960-1993	1960-1988	1960-1983
Panel A: All SES				
Reform mother	-0.078 (0.337)	0.123 (0.247)	0.029 (0.200)	0.129 (0.190)
Obs	429,114	429,114	406,408	347,811
$\bar{y}$ %	25.65	16.81	12.41	8.053
Panel B: Low SES				
Reform mother	0.092 (0.358)	0.259 (0.277)	0.202 (0.265)	0.319 (0.227)
Obs	275,501	275,501	263,373	230,812
$\bar{y}$ %	27.18	17.79	13.28	8.481

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 15-29, 15-19, 20-24 or 25-29. The sample are sons of women born 45-55 who are fully observed for the relevant age bracket of the dependent variable. Robust standard errors, clustered by mother's municipality of birth, in parentheses; all regressions include a full set of mother's birth municipality and mother's birth cohort indicator variables. Panel A includes grandfather's education levels.

Table 2.11: Reform Effects of Mothers on Son's Crime by Type of Crimes

	(1)	(2)	(3)	(4)	(5)
Convicted at age 15-29:	Violent	Property	Drugs	Traffic	Fraud
Panel A: All SES, Obs 429,114					
Reform mother	0.124 (0.110)	0.288 (0.191)	0.016 (0.100)	-0.049 (0.203)	-0.012 (0.097)
$\bar{y}$ %	4.628	8.631	3.122	12.29	3.352
Panel B: Low SES, Obs 275,501					
Reform mother	0.117 (0.144)	0.539** (0.228)	0.095 (0.130)	-0.092 (0.247)	0.124 (0.112)
$\bar{y}$ %	5.053	9.230	3.111	13.42	3.572

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 15-29 for one of the specified crime types. Robust standard errors, clustered by mother's municipality of birth, in parentheses; all regressions include a full set of mother's birth municipality and mother's birth cohort indicator variables. Panel A includes grandfather's education levels.

### 2.7.3 Children Crime Estimations Including Trends

Table 2.12: Age Specific Reform Effects on Son's Crime

	(1)	(2)	(3)	(4)
Convicted at age:	15-29	15-19	20-24	25-29
Cohorts observed:	1960-1993	1960-1993	1960-1988	1960-1983
Panel A: All SES				
Reform father	-0.800*** (0.276)	-0.632*** (0.221)	-0.345* (0.197)	-0.196 (0.208)
Obs	409,083	409,083	364,521	282,305
$\bar{y}$ %	23.53	15.69	11.69	7.863
Panel B: Low SES				
Reform father	-0.571 (0.356)	-0.481* (0.289)	-0.244 (0.263)	0.104 (0.269)
Obs	261,014	261,014	235,478	186,858
$\bar{y}$ %	25.09	16.81	12.44	8.259

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results are scaled by 100. The dependent variables are indicator variables for having ever been convicted between the ages of 15-29, 15-19, 20-24 or 25-29. The sample are sons of men born 45-55 who are fully observed for the relevant age bracket of the dependent variable. Robust standard errors, clustered by father's municipality of birth, in parentheses; all regressions include a full set of father's birth municipality and father's birth cohort indicator variables. In addition all estimations include father's cohort trends that are specific to father's birth municipality group. Panel A includes grandfather's education levels.

Table 2.13: Reform Effects on Prison Sentences for Sons

	(1)	(2)
Sons of men 45-55:	All SES	Low SES
Reform father	-0.015 (0.099)	0.108 (0.138)
Obs	408,021	261,014
$\bar{y}$ %	2.385	2.683

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Effects scaled by 100. The dependent variable for is an indicator variable for having ever been convicted for a prison sentence between the ages 15-29. Robust standard errors in parentheses clustered by father's birth municipality. Includes father's cohort and father's birth municipality indicator variables, and father's birth municipality specific cohort trends. Column (1) includes grandfather's education levels.



## Chapter 3

# Alcohol Policy and Crime-Age Profiles

### 3.1 Introduction

In order to understand the drivers of criminal behaviour alcohol consumption has been identified as one important contributing factor for example to promote violent crimes. In the US the Bureau of Justice Statistics (2004) reports that 33 percent of state prisoners reported to have been under the influence of alcohol at the time of the offence in 2004, with even higher shares reporting alcohol influence for violent crimes and public disorder. The Crime Survey for England and Wales (2013) reports that 47 percent of victims of violent incidents perceived their offender(s) to be under the influence of alcohol, with similar rates since 2001. Around the world governments have identified the important role that alcohol plays for criminal activity and alcohol policies have become the focus of a wide range of crime prevention policies. In the US minimum legal drinking age laws are hotly debated. Opponents to these laws argue that a reduction in the age-cut off would reduce consumption and crime, while proponents provide evidence in the other direction (Carpenter and Dobkin (2011)). In recent years laws on driving under the influence has seen major changes in the US and Europe. Finally, laws governing mandatory opening hours of bars, restaurants and nightclubs as well as retail stores are also popular policy instruments. This chapter focuses on a liberalization of alcohol stores in Sweden.

Sweden is traditionally very strict on alcohol regulation with a state owned alcohol monopoly regulating all alcohol retail sales and high alcohol taxes. Alcohol policies

driven by public health and crime prevention concerns are not news there. Already in 1981 the closing of the state owned alcohol monopoly stores on weekends was driven by such concerns. The return to more liberalized opening hours in 2000-2001 could be seen in the light of more recent policies that aim to reduce alcohol consumption by liberalization. One such example of a major liberalization of alcohol policy with the intention to directly reduce crime and public disorder, is the UK Licensing Act (2003). Though its effect on alcohol and crime (Hough and Hunter (2008)) and on actual opening hours (Humphreys and Eisner (2010)) are still debated. In this policy debate arguments for a liberalization of alcohol policy, such as liberalized opening hours and licenses, opposes more traditional policies that restrict the access to alcohol partly (blue laws) or completely (MLDA). Up to date there is no consensus on whether stricter or more liberal policies are more effective in reducing consumption and crime.

Overall, the existing literature has often focused on one or two types of crimes to evaluate an alcohol policy, sometimes only considering a narrow age bracket, or crime in the aggregate. Evidence on the well established age-crime profiles shows that crime varies substantially across ages and types of crimes which implies that the effects of alcohol on crime are likely to vary by age and types of crimes too (Farrington (1986), Moffitt (1993), and Sampson and Laub (2003)).

In this chapter, I exploit a temporal liberalization of alcohol availability in Sweden to analyse how crime changes by age and type of crime. In 2000-2001 the Swedish government performed a policy experiment that expanded opening hours of the alcohol monopoly stores to open on Saturdays. This expansion corresponds to an increase of the amount of hours by 12.5 percent. In my empirical analysis, I use individual register data on the universe of all criminal convictions in Sweden to determine changes in the crime-age profile induced by those liberalized weekend opening hours. The data enables me to get a more complete picture on overall crime - for all ages and a broad range of types of crimes.

The primary data source used in this study are individual register conviction data covering all convictions in Sweden between 1973-2010. For each individual conviction in Sweden during that time the data has an individual identifier, the age of the offender at the time of the offence, the date of the conviction, and the date of crime committed, regional court identifiers, the number of crimes within each individual conviction, and several variables that allow me to identify the type of the main crime for which the individual was convicted (in a particular conviction). The type of crime variables are

based on the Swedish law books, sections of the law, paragraphs etc. through which I define seven crime categories: violent crimes, property crimes, fraud and tax offences, drug crimes, traffic violations that exclude speeding or parking tickets, sex crimes, and a category others that includes all other crimes that cannot be categorized into one of the six. This categorisation, especially for the others category remains constant over time.<sup>1</sup> My data contain all crimes that individuals are subsequently convicted for and the type of crime categorization is only limited by being able to uniquely identifying a type of crime within the law book (for example assault of a husband towards his wife can not be distinguished between assault towards a stranger, both offenders will be convicted based on the same laws concerning assault). Since the reform is on county level, I aggregate the conviction data by county and month for all men, and compute crime rates for each county-month and age per capita of the population of that county-month-age group. From this data, I construct a crime panel for each county, month and age that I split up by types of crimes, for January 1998-December 2009. With this time window the data covers three years pre-policy and eight to nine years after the reform. This crime panel is matched to additional county characteristics. The empirical analysis of the opening hour expansion experiment is based on a difference-in-differences approach exploiting the staggered implementation of the policy across Swedish counties and time. This chapter expands the existing analysis on Sweden's liberalization of opening hours of Grönqvist and Niknami (2011) that was restricted to individuals between the ages of 17-23 and based on a different identification strategy. The emphasis of the study presented in this chapter is to map out the policy impact by age and type of crimes.

I present evidence on three sets of results. First, the effect of the above mentioned alcohol policy on alcohol sales in each county and month. This is based on monthly alcohol sales data from the alcohol monopoly for the years 1997-2010. My results are in line with the previous Swedish studies that find a significant and positive impact of expanding the opening hours on weekends on alcohol sales (Norström and Skog (2003), Norström and Skog (2005), and Grönqvist and Niknami (2011)). The magnitude of the increase in alcohol sales estimated in this study ranges between 6.6-10 percent increase in pure alcohol sold per capita. This translates into an increase of 0.2-0.3 litres of 13% wine per capita per month, or an increase of 0.5-0.7 litres of 5% beer per capita per month. Those effects are robust across various specifications, and persist

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<sup>1</sup>See the Data description on a more detailed discussion of the type of crime categorization.

over several years.

The main contribution of this paper is presented in the second and third set of results. Those are results of the reform on age-crime profiles for overall crime and for the seven crime categories defined. My descriptive evidence and estimation results presented in this paper show an interesting crime-age pattern induced by the policy: increasing the availability of alcohol reduces overall crime rates for teenagers between 16-17 and adults between the ages 30-40, while it increases crime for young adults between 20-28 and later in life between 45-55. This age pattern, in particular for the two younger age groups, appears to be robust to various specifications.

The third set of results in this paper are effects of the alcohol policy by types of crimes and age. Since, I have the universe of all convictions in Sweden in my data I can map out the impact on a very broad range of types of crimes and all ages. The largest effect of the policy appears to be on drug related crimes. Descriptive evidence from other studies and countries indicates that when alcohol is involved in an offence, other drugs are often involved too. For example, the Bureau of Justice Statistics (2004) states "In 7% of alcohol-involved violent incidents known to law enforcement, other types of drugs were also suspected to be involved". The evidence presented in this paper points towards alcohol and drugs being complements for young adults through ages 20 to 30, supported by a large increase in drug related offences for that age group. The strong increase in overall crime observed for this age group, appears to be mainly driven by drug related offences, while somewhat offset by a reduction in property crimes. In contrast, alcohol and drugs appear to be substitutes for teenagers 16-17, which derives from a reduction in drug related offences after the reform. In addition, for teenagers property crimes decreased significantly and weaker effects for traffic crimes, fraud and other crimes contribute to the overall reduction in crime for teenagers as well. The negative effect on middle aged men stems mainly from a reduction in the crime category others and traffic offences. The weakest effect, the increase in crime for men between the ages 45-55 can be mainly attributed towards increased traffic crimes.

Finally, for violent crimes and sex crimes my results are mixed and one cannot draw firm conclusions. Some ages experience a weak increase in violent crimes and sex crimes following the increased availability of alcohol, mostly for the younger ones, but the pattern is not very strong. Overall, these findings support the notion that liberalization of alcohol policies can both increase and reduce crime, depending on

the age of the offenders and the types of crimes considered.

This study aims at better understanding the link between alcohol availability, age and different crime types. It expands the existing literature with a thorough analysis of how alcohol policy can reduce/increase crime by types of crimes and most importantly for all age-ranges. Most of the previous studies have focused on one or two types of crimes onto which the effect of alcohol is being evaluated, while this study can contribute towards a more general discussion of a very broad range of types of crimes for the entire age distribution. It can map out an age-profile for a broader set of crime categories. It contributes towards the more general discussion of how alcohol policy can be used as policy tool to influence crime, and more specifically towards the existing literature using temporal restrictions as identifying strategy.

In contrast to Biderman, De Mello, and Schneider (2010) that looks at restrictions in social settings like bars this study estimates the impact of a temporal liberalization of alcohol monopoly store opening hours. In Sweden alcohol can only be purchased in those stores or in restaurant and bars. However, for example in 2009 only 12 percent of total alcohol consumed was bought in restaurants or bars and the remaining alcohol consumed was purchased in the monopoly stores (in addition there is some consumption based on exported goods from EU countries but estimates are hard to get by).

In comparison to the age-restriction literature, the liberalization considered removes a temporal restriction on all individuals. Age-laws completely restrict access to alcohol for certain age groups, while in my case individuals could previously purchase alcohol, it is just being made easier to do so on the weekends.

Drug crimes appear to be of great importance when considering alcohol related policy interventions, contributing towards the discussion of the interaction between these two. There does not seem to be a consensus in the existing literature on whether alcohol and drugs should be seen as complements or substitutes. My heterogeneous results by age and type of crime support the idea that the answer crucially depends on the type of policy intervention considered, the target group, and the age of this group.

This chapter is organised as follows. The next section reviews the relevant literature. Section 3.3 describes the institutional background of the Swedish alcohol monopoly and the policy. Section 3.4 describes the data used, provides descriptive evidence on alcohol and crime as well as lays out the empirical strategy employed. In

section 3.5, I present the main results for total crime by age and disaggregated results by types of crimes and age. The end of this section also presents various robustness tests. Finally, Section 3.6 discusses the results and concludes.

## 3.2 Literature Review

A major difficulty in the literature on alcohol consumption and crime lies in establishing causality. Pharmacological and psychology studies show that alcohol reduces inhibition and attention, impairs control, judgements, and reflexes, and also induces aggressive behaviour (see McClelland, Davis, Kalin, and Wanner (1973)). Extreme emotions and aggression in particular might play a role in violent crimes. However, while alcohol has been documented to increase aggression levels in controlled laboratory experiments there still remain doubts about causality (Lipsey, Wilson, Cohen, and Derzon (1997)).

Omitted variables bias is a major concern in the alcohol policy and crime literature, both through omitted unobserved regional factors and unobserved individual heterogeneity. Unobserved neighborhood characteristics such as regional alcohol tax rates, regional alcohol outlet density, attitudes towards alcohol policy, attitudes towards crime punishment, unobserved policies, and more generally neighbourhood quality, might be correlated with alcohol policies and crime outcomes and thereby confounding the estimates. Similarly, alcohol consumption and determinants of crime might be correlated due to omitted unobserved individual characteristics such as risk preferences, mental health problems etc.

Another econometric concern is non random selection in both arrest and victimization data. If offenders are more likely to be arrested or victims are more likely to be victimized when under the influence of alcohol the effect of alcohol policy on crime could be overestimated. Related to this issue, is to disentangle whether alcohol policy affects crime committing, crime victimization or both.

A concern mostly relevant for the interpretation of the results of alcohol policy on crime is whether alcohol policies change alcohol consumption per-se that leads individuals to commit more or less crime or whether those policies shift the social interaction structure. The estimates of alcohol policy on crime must be interpreted as a combination of changing the amount of alcohol consumed and the changes in the social interaction of intoxicated individuals, this applies to studies using MLDA, bar

opening hours, outlet density and opening hours.

Finally, how alcohol policies shifts alcohol consumption for different population groups, the extent (intensity) or conditions of this consumption (locally for example) remains unknown. Especially, when crime outcomes are concerned, the intensity at which individuals consume alcohol and how this intensity changes in response to alcohol policy is an important margin. More generally, whether such policies only have a temporal impact on consumption and crime, or whether it is of a more permanent nature is also not well established. One would also expect interactions of such alcohol policies and police enforcement.

In the economics literature, the documentation of causality between alcohol and crime is weak in general with a few exceptions. Following the classification by Carpenter and Dobkin (2010) one can distinguish those studies by the type of alcohol policy being used (1) variation or changes in excise tax on alcohol, (2) spatial restrictions on alcohol outlet density and availability, (3) age-based restrictions, like minimum legal drinking ages, (4) temporal restrictions on alcohol sales, and (5) combination of those.<sup>2</sup>

The first category on alcohol taxes, inevitably belongs to studies that aim to estimate the price elasticity of alcohol demand. Economic theory predicts that tax induced increases in the price of alcohol should decrease consumption. The parameter measured in those studies could be interpreted as the impact of a direct change in the price of alcohol on crime. There does not appear to be a consensus on this question. The literature survey of Grossman, Sindelar, Mullahy, and Anderson (1993) summarizes the findings to be an elastic long-run, but inelastic short run alcohol demand (suggesting that this might be due to the addictive nature of alcohol, p. 215). Within this set of taxation studies, there are various studies that attempt to use alcohol excise tax variations and its impacts on crime. Based on cross-sectional variation they provide merely correlational evidence. For example, Markowitz (2005) finds that increased beer taxes in England and Wales are correlated with a reduction in assaults and none in rape using victim data. On emergency hospitalization data in England and Wales, Matthews, Shepherd, and Sivarajasingham (2006) finds that the real price of beer is negatively correlated with violent injuries. Due to the cross-sectional data and the type of crime data used in these studies they cannot address

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<sup>2</sup>For an extensive summary of the existing literature based on this classification by type of alcohol policy used see Carpenter and Dobkin (2010).

the omitted variable or the non-random selection issues mentioned above.

Studies that use the spatial differences in bar and outlet density measure a different parameter. Bar density is related to social alcohol consumption, which in itself might contribute to more violent incidents (even without the alcohol). At the same time the density of bars and/or outlets relates to how easily alcohol is available in a region. This means the interpretation of the parameter could be how a reduction in the fixed costs to obtain alcohol (distance to outlet or bar) affects the total price of alcohol, thereby affecting consumption and crime. Assaultive violence and bar/outlet density was shown for example by Scribner, MacKinnon, and Dwyer (1995) to be positively correlated. Those types of studies have attempted to address the omitted unobserved regional characteristics mostly by including regional control variables, others have attempted to address the econometric issues with instruments for alcohol outlets, or event type studies (see Teh (2007)).

Studies on age-based restrictions (3) use more solid identification strategies to find a causal impact of alcohol on crime and emphasise the sensitivity of young adults to respond to alcohol policies. The parameter estimated in those studies is the effect of completely removing a full restriction on alcohol consumption for young adults at a certain age. This can be seen as affecting the total price of alcohol through changing the costs of obtaining it. Convincing evidence from a regression discontinuity design that exploits the age-discontinuity of MLDA in the US come from Carpenter and Dobkin (2009). They show mortality rates, motor vehicle deaths or alcohol related deaths increase around the age cut-off at 21. The variation in MLDA laws across US states has been linked to alcohol consumption increases by 57 percent more days and increased arrest rates for those just above the age of 21 (Carpenter and Dobkin (2010)). Finally, zero-tolerance underage drunk-driving laws directly aim at the issues of drinking and traffic and has been shown to change the age-structure of DUI arrests, a decrease in property crimes and no effect on violent crimes for young adults (Carpenter (2005) and Carpenter (2007)). These types of studies address the omitted variables issues by exploiting a regression discontinuity design around the age cut-off. The issues to not being able to disentangle social interaction and increased consumption, and that intoxicated offenders or victims might be more likely to be arrested or victimized remain. There is no reason to believe that increasing alcohol availability at the age of 21 has the same effect on consumption and crime if the proportional increase happened at other ages. This external validity problem



regarding the age of the individuals considered could be seen as one disadvantage, though, since crime peaks around the age of 18-21 these studies measure the impact of alcohol policy on crime at a point in the life-cycle when crime is most prevalent.

Targeted sales restrictions that temporarily restrict sales in bars or stores (4) impose restrictions on the population as a whole but only in a specific setting. Sales restrictions in bars or restaurants, often called dry laws, restrict social or recreational consumption. The parameter measured in such studies is how changes in the price to obtain alcohol (change in the fixed costs) affects crime. These types of studies are able to address the omitted variable problem by including region fixed effects, and thereby accounting for time-invariant omitted variables that are likely to be correlated with the alcohol policy and crime in that region. Using difference-in-difference strategies, these studies can implement a convincing pre-post policy design. Convincing evidence on the effectiveness of changes in the mandatory closing hours of bars and restaurants on crime reduction in Brazil is provided in Biderman, De Mello, and Schneider (2010). The authors document a large reduction of about 10 percent in homicides induced by the mandatory night closing hours for bars between March 2001 and August 2004 in São Paulo Metropolitan Area, with similar results for battery and accident related deaths. The authors also find a reduction in bar consumption of alcohol (by large magnitude of 50-70 percent).

Sales restrictions on stores in contrast impose a restriction on times at which individuals can buy alcohol for consumption at home. For example, restricted opening hours on Sundays in Canada shows a reduction in alcohol sales on the day of the restriction (Carpenter and Eisenberg (2009)). From Sweden, earlier studies of Norström and Skog (2003) and Norström and Skog (2005) document a significant increase in alcohol sales following the Saturday opening hour expansion of the alcohol stores, but no effect on crime (measured by reported assaults) or on aggregated health indicators. More recently, Grönqvist and Niknami (2011) use individual conviction register data and the same opening hours expansion. They compare conviction rates in counties where the stores remained closed between individuals below and above the national drinking age of 20 that is the age group 17-18 (excluding 19 year olds) compared to 20-23 to the difference in counties where stores were opened on Saturdays. One concern with this approach is to use teenagers below the age of 20 as control groups. In Sweden individuals are allowed to purchase alcohol in the alcohol stores from the age of 20 onwards, while alcohol purchase and consumption on-premises in bars, night-

clubs and restaurants is allowed from the age of 18. By assigning teenagers below 20 as being not treated by the policy, the authors ignore spill-over effects of older siblings or friends who can buy alcohol for them. They find an increase in property crime of 11.6 percent, none on total or violent crimes for men aged 20-23 living in counties where the expansion was first introduced.<sup>3</sup>

The last set of studies reviewed here, relates to several of the above categories, but distinguishes itself by the types of crimes considered. These studies are concerned with the complementarity/substitutability of alcohol and drugs. The evidence in this literature is mixed, on the one hand, there is support for alcohol and marijuana (and other harder drugs) to be complements.

For example, Yörük and Yörük (2011) and Yörük and Yörük (2013) find a negative spill over effect from alcohol towards marijuana consumption using a regression discontinuity design around the MLDA age cut-off with data from the NLSY97. Their findings translate into an increase in marijuana use of about 5.5-7 percentage points around the 21 years age cut-off.<sup>4</sup> In contrast, Crost and Guerrero (2012) finds support for the opposite. Using the 2002-2007 National Survey of Drug Use and Health (NS-DUH) in the US they find that alcohol consumption at the MLDA cut-off increases by 16 percent and reduces marijuana consumption by 10 percent supporting the view that alcohol and marijuana are substitutes.<sup>5</sup>

Anderson and Hansen (forthcoming) is relevant for my findings on traffic violations and drugs combined. Based on the implementation of medical marijuana laws (MML) in the US their findings provide evidence for MMLs to reduce traffic fatalities in total (by 9 percent), strongest among young adults, and even more so for traffic accidents involving alcohol (by 12 percent for any alcohol involvement and 14 percent reduction for high alcohol level involvement). They also speak towards age-specific effects, finding little impact of MMLs on minors and an especially strong reduction in alcohol consumption for ages 20 through 29 years. They argue that an increase in marijuana consumption reduces alcohol consumption which reduces traffic fatal-

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<sup>3</sup>Additional findings of their study: On Saturdays total crime of 20-23 year olds increased by 18.7 percent in the early treated counties, and the effect is stronger with an 21 percent increase for those men with low compulsory schooling grades.

<sup>4</sup>As part of their study Yörük and Yörük (2011) also finds that MLDA increases the probability of alcohol consumption for those after the age-cut-off (13 percentage points), increase in number of days in a month spent drinking (1.7 more days), increase of probability of binge drinking (10 percentage points), but they find no increase in the number of drinks consumed in a day.

<sup>5</sup>Further evidence on the substitutability between alcohol and marijuana comes from Chaloupka and Laixuthai (1997), DiNardo and Lemieux (2001). More evidence on the complementarity: Farrelly, Bray, Zarkin, Wendling, and Pacula (1999), Williams, Pacula, Chaloupka, and Wechsler (2004).

ities (under alcohol influence), supporting the idea that marijuana and alcohol are substitutes.

The majority of the existing studies measure how changes in the cost of obtaining alcohol influences crime. Lifting of age-restrictions reduces the costs of illegally obtaining alcohol (psychic costs of breaking the law, legal consequences warnings etc.) can be seen as a different cost from costs incurred by changing regional bar/outlet store availability from having fewer stores in their neighbourhood (longer distances to drive), and changing opening hours that restrict individuals to purchase alcohol within set opening hours. Common in all those policies is the change in the costs of obtaining alcohol, that indirectly contributes towards the price of alcohol. The only work that directly changes the monetary price of alcohol are the tax studies. At the same time the social interaction component has to be considered when interpreting the results.

### 3.3 Institutional Details of the Policy

#### 3.3.1 Background of the Swedish Alcohol Monopoly

To understand the policy that this study focuses on, one has to go back to explain how alcohol sales have been historically organised in Sweden. Alcohol sales in Sweden are strictly regulated by the government and since 1954 a state alcohol monopoly system is in effect. Any alcoholic beverages with alcohol content above 3.5% by volume are only sold in state monopoly alcohol retail stores called *Systembolaget*. While it is legal to consume alcohol on-premises in bars and restaurants from the age of 18, the minimum legal age to purchase alcohol in off-premises stores is 20 which is very strictly enforced with the requirement to show ID cards.<sup>6</sup> Regular grocery stores are only allowed to sell light beers with an alcohol content of less than 3.5% by volume.

Alcohol excise taxes in Sweden are higher than in any other EU country. In the relevant period 1998-2009 ethyl alcohol was taxed at 501.41 SEK (GBP 37.5) per litre, wines with 8.5-15 percentage of alcohol at 27.20-21.58 SEK (GBP 2.03-1.61) per litre, beers exceeding 3.5 percentage of alcohol were taxed at 1.47-1.66 SEK (GBP 0.11-0.12) per litre.<sup>7</sup> Excise taxes on wines below 15 percentage of alcohol were reduced

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<sup>6</sup>Anybody who looks younger than 25 will be asked to show their ID card when purchasing alcohol. This also applies to anybody in the company of the purchasing person in the store.

<sup>7</sup>Computed at the average exchange rate for the years 1998-2009: 1 SEK=GBP 0.0747 (1 SEK=EUR 0.1082 and 1 SEK=USD 0.1266).

by 18.8 percent in 2002, with a second reduction of 2.2 percent in 2008. For all other alcohol types (spirits, stronger wine, and beer<sup>8</sup>) excise taxes remained constant. The 25 percent VAT tax rate on all alcohol remained constant since 1991. Potential impacts of the wine excise tax rate changes are discussed in Section 3.4.3.

The opening hours of the alcohol stores *Systembolaget* are also decided and regulated by the Swedish government.<sup>9</sup> In 1981 the government decided to conduct an experiment between June and September 1981, in which all alcohol stores were to close on Saturdays. The experiment was evaluated based on the effects on alcohol sales, drunkenness, domestic and public disturbances (Olsson and Wikström (1982)). They find that the 1981 experiment decreased total alcohol sales and the considered aggregated crime outcomes relative to the same period in the previous year. The reduction was more pronounced on Saturdays. Partly based on the results of the experiment, the Swedish government decided in Spring 1982 to permanently close alcohol stores on Saturdays starting from 1 July 1982. Stores remained closed on weekends until the re-opening on Saturdays after another policy experiment starting in 2000. Data limitations in my crime data prevent me from being able to explore this first opening hours reform in 1982. It is the second experiment that started in 2000 which this study focuses on.

### 3.3.2 The 2000-2001 Weekend Opening Hours Liberalization

The Swedish parliament decided to open alcohol retail stores on Saturdays in 6 out of 21 counties as part of an experiment in February 2000. The aim of the experiment was to evaluate and assess the effect of the liberalized opening hours before expanding it to the rest of the country. The experimental counties Jämtland, Norrbotten, Skåne, Stockholm, Västerbotten and Västernorrland comprise 3.8m individuals which corresponds to about half of the entire Swedish population. Those counties include South Sweden (including Sweden's third largest city Malmö), the Stockholm region (Sweden's largest metropolitan area), as well as three counties in the North (the least densely populated area). The 2000 policy experiment was initially evaluated in the official report by Norström and Skog (2003), who defined the treatment regions to be non-neighbouring counties of the experimental counties. They reported (i) an increase

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<sup>8</sup>The excise taxes on beer increased in 2008 by 12.9 percent.

<sup>9</sup>There have historically been some exceptions with short alcohol sales experiments, for example the strong beer sales experiment in parts of Sweden between November 1967 and July 1968 (see Nilsson (2012) and Nilsson (2008)).

in alcohol sales, (ii) no increase in the number of assaults reported to the police, (iii) no change in various health indicators for the experimental regions compared with the treatment regions after February 2000. Based on this official report, the Swedish parliament voted in spring 2001 for the country wide introduction of the liberalized opening hours to be implemented in the remaining 15 counties in July 2001. Since July 2001 alcohol stores are open on Saturdays across the whole country. The second stage of the experiment was evaluated by Norström and Skog (2005), who expanded on their initial evaluation to find that alcohol sales increased by equally much in the second stage of the experiment, with no effects on assaults after introduction neither for early nor late introducing counties, but an effect on drunk-driving in early introducing counties and none in late introducers.

## 3.4 Data, Empirical Method and Descriptives

### 3.4.1 Data Sources

#### Crime Register Data

I exploit detailed individual conviction register data from Sweden, provided by *The Swedish National Council for Crime Prevention (Brå)* that contain all convictions in Sweden between 1973 and 2010. Every single conviction listed identifies the individual who was convicted with a unique personal identifier number. In addition every single conviction observation lists: the total number of crimes the individual is being convicted for in that particular conviction, the main offence of that conviction defined as the most severe violation within the conviction, the type of crime of the main crime, the date of the conviction, the date when the crime was committed, the age at offence, and a court identifier number.

The main crime within each conviction specifies the main crime rate and the number of crimes stated in each conviction is used to compute total crime rates. Types of crimes are detailed in several variables that specify the law book, section, chapter, paragraph, moment, piece and point in the Swedish law that the crime was convicted for.<sup>10</sup> Variables specifying the types of crimes are available only for the main crime. I define seven types of crime categories: violent crimes (*violent*), property crimes and

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<sup>10</sup>Details of the types of crimes variables in the conviction data in Brå Variabelbeskrivning Lagföringsregistret (2009) and the documentation of coding crime types in Brå Kodning av Brott (2010)

theft (*property*), fraud and tax evasion (*fraud*), drugs offences (*drugs*), sex crimes (*sex*), traffic crimes excluding speeding and parking tickets (*traffic*), and other crimes that can include anything else that does not belong into the latter six categories (*others*). The categorisation of crime types and the coding is based on the documentation provided by Brå Kodning av Brott (2010) as well as the Swedish law books which the variables in the data refer to (i.e. law-book and chapter). The limitations of the crime type variables are for example that I cannot distinguish different types of assaults. Domestic violence that lead to an assault of the spouse and an assault on the street where the offender and the victim are strangers, will both appear in the data as assault, because both offences will be convicted based on the same law.<sup>11</sup> The category summarizing all other crimes that do not fit into one of the six specific types does not grow or shrink over time. It does not include certain crimes in one year that it did not in previous years (and vice versa). Where changes in the Swedish law took place the coding of the type of crime was adjusted for each single change over the relevant period. Figure 3.8 in the Appendix plots the total number of crimes for each crime category by year. One can observe some variation over time, but overall all crimes except traffic, appear stable across time.

The court identifier numbers are matched to municipality or county names that the court serves through which I identify the region where the crime was committed.<sup>12</sup> In general courts deal with crimes that were committed in that region. Court codes are available and can be matched to regional codes for all convictions from 1995 onwards.<sup>13</sup> Exact dates of when the crimes were committed are only provided in about 60-65 percent of convictions in the years 1973-1997, convictions in the year 1998 miss a date of crime only in 10.8 percent. For convictions from 1999 onwards the date of the crime committed are available in all cases. Typically convictions take place 1-2 years after the offence. The time range used for the empirical analysis is determined by these data restrictions and I use all crimes that were committed between 1998 and 2009 and that have subsequently led to a court conviction. This is also the reason for not being able to analyze the reversed policy in 1982 which closed the stores on Saturdays.

The individual conviction data was aggregated to the county-month-age-gender

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<sup>11</sup>In this case Swedish Penal Code, Chapter 3 Section 5.

<sup>12</sup>Specification of regional matches to court codes *Domstolskod* and *Åklagarkod* were provided by brå.

<sup>13</sup>Note year 1995 refers to conviction year.

level to construct a panel data set of crime for men by age for all Swedish counties and for each month between January 1998-December 2009.<sup>14</sup> Crime rates are computed for each county and month based on yearly population size data extrapolated to month level. Population numbers for each county-year-age-gender observation are taken from Statistics Sweden Population data. Rates are computed for the main crimes, crime types for the main crimes and for the total number of crimes.

## County Characteristics and Alcohol Sales Data

To control for confounding factors that are relevant for criminal behaviour I use aggregated data from the publicly available data of Statistics Sweden on employment levels and education. As labour market indicators I use the share of individuals employed in each county and age for each month as well as education levels for each county, age and year.<sup>15</sup> These were matched on to the crime data.

Finally, to evaluate the impact of the alcohol experiment on alcohol sales and consumption I use data from several sources. Monthly alcohol sales data by litres of pure alcohol, total litres and amount in Swedish Krona by county for each month between January 1997 and December 2011, were directly provided by the alcohol monopoly store *Systembolaget*. In my analysis I focus on the monthly alcohol sales data by total litres of pure alcohol, from which I compute the litres of pure alcohol bought per capita in a month. From this measure one can compute how many litres this would correspond to in beer or wine of a certain percentage of alcohol. Monthly alcohol sales data by types of alcohol (beer, wine, spirit and others) from *Systembolaget* is only available for the entire country (not by county) during the relevant period. On county level the data is on yearly level. For descriptive purposes, I use yearly sales data by types of alcohol publicly available at the Swedish National Institute for Public Health (Statens Folkhälsoinstitut).

Statistics Sweden administers a Living Conditions survey, in which alcohol consumption was included in the questionnaires for the years 1995-96 and 2004. I use the published descriptive evidence on changes in the alcohol consumption intensity over time by age groups from the Living Standard Report from Statistics Sweden. Unfortunately, the Living Conditions Survey for Children was first conducted in 2001, which means I cannot infer about alcohol consumption changes of children and teenagers for

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<sup>14</sup>Documentation of variables and description in Brå Variabelbeskrivning Lagföringsregistret (2009)

<sup>15</sup>These aggregated data are available on the Statistics Sweden webpage.

the relevant period.<sup>16</sup>

### 3.4.2 Empirical Methods

The first part of this chapter will establish the impact of the reform on alcohol sales. The estimated difference-in-differences specification to analyze changes in alcohol sales following the expansion of Saturday opening hours of alcohol stores has the form,

$$(3.1) \quad \log(A_{cmy}) = \alpha_1 + \beta_1 T_{cmy} + \gamma_c + \gamma_m + \theta_1 t + w_{cmy}.$$

This equation is estimated based on a monthly panel of alcohol sales for each county  $c$  and month for the years 1997-2009 indicated by  $my$ . Alcohol sales are measured as the logarithm of alcohol sold in litres of 100 percent alcohol per capita of adult population above age 20 in each county and each month,  $\log(A_{cmy})$ .  $T_{cmy}$  is an indicator variable that equals 1 if the policy was implemented in the county  $c$  in month  $m$  and year  $y$ . To control for county specific time invariant characteristics all estimations include county fixed effects ( $\gamma_c$ ). Alcohol sales vary by season and month  $m$ , hence month fixed effects ( $\gamma_m$ ) and linear time trends ( $t$ ) are included. All alcohol estimations presented in this study are based on Prais-Winsten regressions, for which I define time  $t$  as the number of months since January 1997. Using this time variable I define a county-specific time trend ( $t_c$ ) that is used in the robustness exercises instead of the linear trend ( $t$ ). Furthermore, the underlying assumption is a county-specific AR(1) error structure,  $w_{cmy} = w_{ct} = \rho_c u_{ct-1} + e_{ct}$ , where  $e_{ct}$  is a classical error term, and  $w_{cmy}$  is county-specific heteroskedastic and contemporaneously correlated across counties.

To analyze the effects of the policy change in alcohol store opening hours on crime by age I estimate the following panel data specification for county  $c$  in month  $m$  and

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<sup>16</sup>There exists a number of alcohol consumption surveys in Sweden, but unfortunately they either start too late or access is not possible. For example the Monitor Project administered between 2001-2012 by the Center for Social Research on Alcohol and Drugs at Stockholm University (SoRAD) and since 2012 at the Swedish Council for Information on Alcohol and Other Drugs (CAN). The start of the study is too late to be used for this study, and access is not possible for external researchers.



year  $y$  for age  $a$ ,

$$(3.2) \quad y_{cmya}^j = \alpha + \beta_a D(a) * T_{cmy} + \eta_a D(a) + \gamma_c + \gamma_m + \delta_1 X_{cmya} + \delta_2 X_{cmyg} + \theta_c t_c + v_{cmya},$$

where  $y_{cmya}^j$  is the number of crimes of the category  $j$  committed in county-month-year and age  $a$ , per 100,000 of the male population.  $D(a)$  is an indicator variable for age  $a = \{16, \dots, 64\}$  and  $T_{cmy}$  is an indicator variable that equals 1 if the policy was implemented in the county  $c$  in month  $m$  and year  $y$ . The age specific parameter  $\beta_a$  is the parameter of interest that captures the differential changes in crime rates by age before and after the policy. The fixed effects  $\gamma_c$  and  $\gamma_m$  capture permanent differences across counties and monthly variation in crime, respectively. In  $X_{cmya}$  and  $X_{cmyg}$  I control for county-specific time varying and age or age-group specific variables: the share of individuals in the population holding a certain education level in age  $a$  in year  $y$  and the share of gainfully employed men in the population for men in the age-groups 16-19, 20-24, 25-29, etc. Just like the alcohol estimations, I estimate all crime estimations using a Prais-Winsten regression, for which I define time  $t$  as the number of months since January 1998. The underlying assumptions on the county-specific AR(1) error structure,  $v_{cmya}$  are the same as above. Estimations are done for the full sample, which means I include all age groups in the estimation. The results section will present three sets of results for different dependent crime variables: (1) main crime rate, (2) separated by seven types of crimes referring to the main crime, and finally, (3) total crime rate.

### 3.4.3 Descriptive Evidence

#### The Policy Impact on Alcohol Sales

The first important fact to establish for this study is whether the alcohol policy had an impact on alcohol sales, which I will then argue has an impact on criminal behaviour. Table 3.1 shows average monthly alcohol sales per capita in litres of pure alcohol before and after the policy was introduced (and split up for early and late treated counties). The Table also reports the unconditional differences in average alcohol sales before and after. The standard errors of a test of equal means shows that the unconditional means are significantly different. To put the numbers into perspective

relative to litres of wine or beer sold, I transformed the litres of pure alcohol per capita into how many litres of wine with 13 percent alcohol or litres of beer with 5 percent alcohol this would correspond to. Before the reform Systembolaget sold 0.345 litres of pure alcohol per capita per month, this corresponds to 2.7 litres of wine, or 7 litres of beer per capita and month. After the reform this increased to an average of 0.464 litres of pure alcohol, corresponding to 3.6 litres of wine or 9.3 litres of beer per capita per month. The levels are very similar across early and late treated counties. These numbers are averages that also include the more general trend of increases in alcohol consumption over the years. In the empirical analysis all estimations include time trends. Figure 3.1 shows yearly alcohol sales by types of alcohol over the years for entire Sweden. One can observe an increasing trend in both beer and wine sales per capita, while spirits and others (cider or mixed drinks like alcopops) remained constant throughout.

Table 3.1: Average Alcohol Sales Before and After the Policy

Years 1998-2008				
All Counties				
	After	Before	Difference	SE
Average Litres of 100% Alcohol Per Capita Sold	0.464	0.354	0.110***	(0.005)
Equivalent to Litres of 13% Wine Per Capita Sold	3.567	2.725		
Equivalent to Litres of 5% Beer Per Capita Sold	9.275	7.086		
Observations	1,992	1,032		
Early Counties				
Average Litres of 100% Alcohol Per Capita Sold	0.498	0.374	0.124***	(0.011)
Equivalent to Litres of 13% Wine Per Capita Sold	3.830	2.875		
Equivalent to Litres of 5% Beer Per Capita Sold	9.957	7.476		
Observations	642	222		
Late Counties				
Average Litres of 100% Alcohol Per Capita Sold	0.448	0.349	0.099***	(0.006)
Equivalent to Litres of 13% Wine Per Capita Sold	3.443	2.684		
Equivalent to Litres of 5% Beer Per Capita Sold	8.951	6.979		
Observations	1,350	810		

Notes: Average monthly alcohol sales of alcohol monopoly store Systembolaget before and after the liberalization of Saturday opening hours. Average monthly alcohol sales are stated in litres of 100% pure alcohol per capita of adult population above the age of 20. The difference of the unconditional averages are reported, including the standard error of a test of equality of the means. For comparison purposes, those litres of pure alcohol are scaled towards the equivalent in litres of wine with an alcohol content of 13%, and for the equivalent in litres of beer with an alcohol content of 5%. Averages are presented for all counties, and separately for early and late treated counties.

Table 3.2 presents the results from estimating Equation 3.1. There is a large statistically significant increase in alcohol sales following the liberalization of opening hours in the range of 6.6-10.3 percent depending on the specification used. This increase corresponds to 0.023-0.036 litres of pure alcohol per capita and month. Scaling this number of pure alcohol towards 13 percent wine or 5 percent beer, this would corre-

spond to an increase of 0.18-0.28 litres of wine or 0.47-0.73 litres of beer per capita per month. The baseline specification covers all counties and the time period between January 1997 to December 2008, which includes the first and the second stage of the policy experiment. Column (2) to (7) in Table 3.2 perform various robustness checks of the baseline results. Column (2) includes county specific linear time trends not altering the effect.

In 2004 EU laws required Sweden to abolish its import restrictions on alcohol from other EU countries. Column (3) only includes data up until 2003. All other EU countries, and in particular neighbouring countries such as Germany, Denmark, and Finland, have lower excise taxes on alcohol. One would expect a decrease in alcohol sales within Sweden due to the trade liberalization (increased opportunity to bring or buy alcohol from traveling to other EU countries) and hence exclusion of post 2003 alcohol sales should increase the estimated coefficient. However, the reverse is true and the estimate decreases by excluding years 2004 and beyond. As Figure 3.1 suggests the drop in alcohol sales in 2004 was merely restricted to beer sales, not wine or spirits, and was only restricted to 2004 and somewhat 2005 and seems to not have long-lasting impacts on alcohol sales.

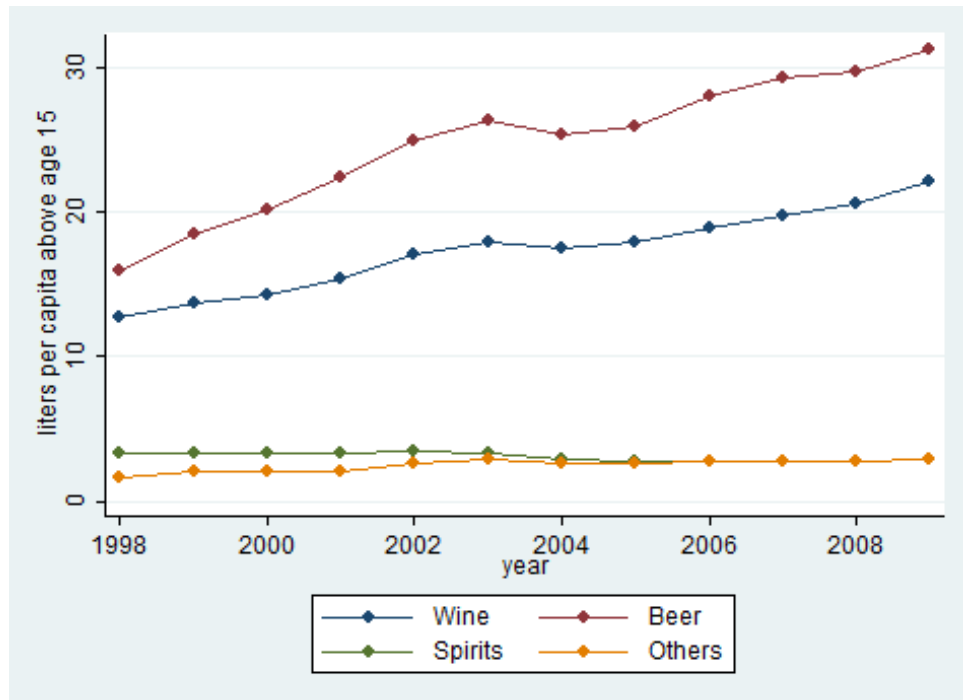
Table 3.2: The Effect of Liberalized Opening Hours on Alcohol Sales

Dependent Variable: Log(Litres of 100% Alcohol Sold per Capita in Month)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	County Specific	Before 2004	Before 07/2001	No Border Counties	No South Sweden	No Border No South
Policy	0.091*** (0.009)	0.097*** (0.008)	0.066*** (0.007)	0.074*** (0.005)	0.077*** (0.004)	0.077*** (0.005)	0.081*** (0.004)
Pre-Policy Average Litres 100% Alcohol Per Capita	0.354	0.354	0.354	0.354	0.368	0.358	0.376
Pre-policy Litres 13% Wine Per Capita	2.725	2.725	2.725	2.725	2.833	2.753	2.889
Pre-policy Litres 5% Beer Per Capita	7.086	7.086	7.086	7.086	7.366	7.158	7.512
County, Month Fixed Effects and Linear Trends	y	y	y	y	y	y	y
County Specific Linear Trends	n	y	y	y	y	y	y
Counties	All	All	All	All	No border	No Skane	No Border
Observation Period	97-08	97-08	97-03	97-01	97-01	97-01	97-01
$R^2$	0.877	0.893	0.898	0.898	0.886	0.895	0.881
Observations	3,024	3,024	1,764	1,134	702	1,080	648

Notes: Significance levels \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. The dependent variable is the logarithm of litres of 100% alcohol sold per capita in a county and month by the alcohol monopoly store Systembolaget. Robust standard errors in parentheses, clustered by county. The sample period runs from 01/1997-12/2008 in column (1)-(2), from 01/1997-12/2003 in column (3) and from 01/1997-06/2001 in columns (4)-(7). The Policy indicator variable equals 1 from 02/2000 onwards for early treated counties, and equals 1 from 07/2001 for late treated counties and zero otherwise. All specifications include county and month fixed effects and linear time trends. Specifications (2)-(7) include county specific linear time trends. Columns (1)-(4) include all Swedish counties, column (5) excludes counties that border early treated counties, column (6) excludes Skane which borders Denmark, and column (7) excludes both. All estimations are based on Prais-Winsten regressions, for which I define time t as the number of months since January 1997. The underlying assumption is a county-specific AR(1) error structure,  $w_{ct} = \rho_c u_{ct-1} + e_{ct}$ , where  $e_{ct}$  is a classical error term, and  $w_{ct}$  is county-specific heteroskedastic and contemporaneously correlated across counties.

Specifications (4)-(7) all include county specific time trends and restrict the sample period to end before the second stage of the policy started, January 1997 to June 2001. The overall effect from the first stage of the policy increases alcohol sales by 7.4 percent (0.026 litres of pure alcohol per capita and month) in those counties that implemented early. Border shopping within Sweden from late to early treated counties or from Skåne (South Sweden) into Denmark could also have an impact on the results. Excluding bordering counties to the early treatment counties increases the coefficient somewhat to 7.9 percent. Excluding Skåne (Southern most county in Sweden bordering Copenhagen, Denmark) slightly increases the coefficient to 7.7 percent. Relative to column (4) these estimates point toward robustness of the estimated results. Border shopping does not appear to be important.

Figure 3.1: Alcohol Sales in Litres Sold per Capita Aged Above 15 by Alcohol Type



Notes: Figure shows yearly number of litres sold by the alcohol monopoly store Systembolaget per adult population above the age of 15 for beer, wine, spirits and others between 1998 and 2009. The category others include cider and mixed alcoholic beverages such as alcopops. Source: Statens Folkhälsoinstitut, Databas Alkohol Konsumtion.

Comparing my results to the existing Swedish studies of Grönqvist and Niknami (2011) and Norström and Skog (2005) I find that the results of my preferred specification are larger. The difference to the other studies stems from the fact that they focus on a more narrow time window only up until the second stage of the experiment was implemented as well as restricting the control group to the sample of non-bordering counties to the first treated counties (see Table 3.2 columns (4)-(7)). When choosing

the same sample as Grönqvist and Niknami (2011), my results are identical to theirs.

The dynamics of the changes are presented in Table 3.3 with all specifications including county and month fixed effects and county specific linear time trends. Column (1) splits up the treatment variable into early and late treatment, indicating increases in alcohol sales per capita for both types of counties by around 9 percent. The variables 0-1 years, 1-2 years, 2-3 years, and 3-4 years indicate the time elapsed since implementation. The first year after implementation saw a 6.6 percent increase in sales for both types of counties, a 8.6 percent increase during the second year after implementation, 6.3 percent for the third, and only during the fourth year did the effect level off to a 1.6 percent increase that is not statistically significant anymore. Overall, there appears to be a strong and long-lasting increase in alcohol sales for both types of counties.

Table 3.3: Dynamics of Alcohol Policy Effects on Alcohol Sales

Dependent variable: Log(Litres of 100% Alcohol Sold Per Capita in Month)		
	(1) early vs late	(2) time elapsed
Early Treatment	0.095*** (0.014)	
Late Treatment	0.097*** (0.011)	
0-1 years		0.066*** (0.006)
1-2 years		0.086*** (0.009)
2-3 years		0.063*** (0.008)
3-4 years		0.016 (0.012)
Obs	3,024	3,024
$R^2$	0.897	0.893

Notes: Significance levels \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . The dependent variable is the logarithm of litres of 100% alcohol sold per capita in a county and month by the alcohol monopoly store Systembolaget. Robust standard errors in parentheses, clustered by county. The sample period runs from 01/1997-12/2008. In Specification (1), the Early indicator variable equals 1 from 02/2000 onwards for early treated counties, and zero otherwise. The Late indicator variable equals 1 from 07/2001 for late treated counties, and zero otherwise. In Specification (2), the indicator variable 0-1 years equals 1 during the first year after treatment, and zero otherwise, the indicator variable 1-2 equals 1 from 1 to 2 years after treatment, etc. All specifications include county and month fixed effects and county specific linear time trends. All estimations are based on Prais-Winsten regressions, for which I define time  $t$  as the number of months since January 1997. The underlying assumption is a county-specific AR(1) error structure,  $w_{cmy} = w_{ct} = \rho_c u_{ct-1} + e_{ct}$ , where  $e_{ct}$  is a classical error term, and  $w_{cmy}$  is county-specific heteroskedastic and contemporaneously correlated across counties.

As emphasized above, my analysis is focused on the heterogeneity of the reform effect on crime with respect to age. Optimally, I would like to estimate the effect of the liberalized opening hours on alcohol consumption by age, but data limitations prevent me from doing so. I will use the descriptive statistics published in the Living Conditions Report of Statistics Sweden to show some differences in changes in alcohol consumption intensity across age groups. The Living Conditions Survey conducted by Statistics Sweden contains questions on alcohol consumption for the years 1995-96 and 2004 that is administered for adults above 20. Table 3.4 presents the percentages of men reporting one of four intensities of alcohol consumption and how those changed between the two surveys. No consumption is defined as reporting to have consumed no alcohol within the past 12 months, low consumption for men is defined as less than approximately 2 bottles of wine per week, medium consumption between approximately 2 and 2.5 bottles of wine per week, and high consumption above approximately 2.5 litres of wine per week.

Table 3.4: Alcohol Consumption Intensity for Men by Age Reported in 2004 and 1995-96 Survey, in Percent of Surveyed Men

	Percent Reporting No Consumption			Percent Reporting Low Consumption			Percent Reporting Medium Consumption			Percent Reporting High Consumption		
	2004	95-96	Diff	2004	95-96	Diff	2004	95-96	Diff	2004	95-96	Diff
Total	10	14	-4	55	60	-5	19	14	5	15	12	3
Ages												
20-34	6.2	6.7	-0.5	52.4	60.5	-8.1	21.4	17.7	3.7	20.0	15.1	4.9
35-44	6.8	10.3	-3.5	55.4	59.7	-4.3	21.2	17.6	3.6	16.6	12.4	4.2
45-54	9.1	10	-0.9	55.0	62.4	-7.4	20.4	15.5	4.9	15.5	12.1	3.4
55-64	8.7	14.4	-5.7	56.7	62.4	-5.7	20.2	12.4	7.8	14.4	10.8	3.6
65-74	16.2	23.7	-7.5	60.9	60.7	0.2	14.7	7.4	7.3	8.2	8.2	0
75-84	29.1	34.9	-5.8	55.9	53.3	2.6	8.2	6.4	1.8	6.8	5.4	1.4

Notes: Percent of men reporting one of the four levels of alcohol consumption intensity in Living Conditions Survey 1995-96 and 2004, and the change between the surveys. Definitions of alcohol intensity for men: no consumption: no alcohol consumption at all in the last 12 months, Low consumption: at most 12.6 cl 100 % alcohol per week, which is at most 2 bottles of wine per week, middle consumption: between 12.6 cl and 21 cl of 100% alcohol, between 2 and 2.5 bottles of wine per week, high consumption: 21 cl 100% alcohol, which is more than 2.5 bottles of wine per week. Source: Living Conditions Report no 114, Use of alcohol and tobacco, Official Statistics of Sweden, Statistics Sweden 2007 (Statistiska centralbyrån (2007)).

More general observations from these descriptives are that for all age groups the proportion of men who report no consumption or low consumption decreased in 2004 relative to the 95-96 survey, while reporting medium and high consumption increased for all age groups. The highest proportion of those reporting high alcohol consumption are men aged 20-34, followed by men aged 35-44, and further decreasing with age in

both surveys. In this high consumption groups the largest increase between 95-96 and 2004 is reported for men aged 20-34 at 4.9 percent, followed by 35-44 year olds at 4.2 percent and at around 3 percent for older ages until 65. Medium consumption appears to be more equally distributed across ages, with around 20 percent of men between the ages 20-64 reporting medium consumption levels in 2004, though changes between 95-96 and 2004 appear to be largest for older age groups between 55-74 year olds at over 7 percent, 5 percent for men aged 45-54 and only around 3 percent for the younger ages. A higher percentage of older ages reports no alcohol consumption compared to the younger age groups, however, the largest reduction over time in the no alcohol consumption category seem to happen for older men above 55 years.

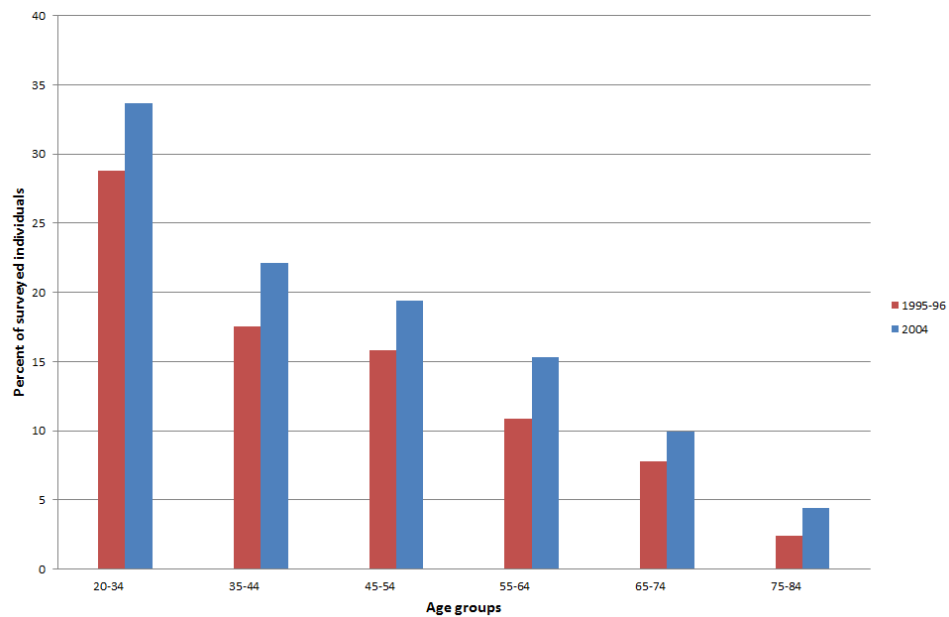
Another relevant statistic is binge drinking reports. Unlike a certain average weekly consumption which could be spread out over several days, binge drinking is defined as drinking more than one quarter of a bottle of spirit, or equivalently more than 1 bottle of wine at one single point in time at least once every month. Figure 3.2 presents percentages of men who report binge drinking by age groups and for both survey years. Similarly to medium and high intensity drinking, binge drinking increased for all age groups between 95-96 and 2004. In 95-96 around 28 percent of men aged 20-34 reported to binge drink and that increased to around 33 percent in 2004. While, the percentage of men reporting binge drinking declines by age, still 23 percent of men aged 35-44 report binge drinking in 2004 (up from 17 percent in 95-96). For the older age groups, 18 percent of men aged 45-54 report doing so, 15 percent of 55-64 year olds, 10 percent of 65-74 year olds, and less than 5 percent of 75-84 year olds. All of the older age groups saw an increase of about 2-3 percentage points since 95-96.

While, I cannot examine alcohol consumption responses by age, the analysis for total sales has shown an increase following the reform. The descriptives from the Statistics Sweden report point towards high levels of high intensity and in particular binge drinking activities of men aged 20-45, with largest increases since 95-96 for the youngest, men aged 20-34. Thus, one might expect the largest effect of alcohol liberalisation on crime for this age group if high intensity alcohol consumption leads to increased criminal activity. Unfortunately, there are no alcohol consumption surveys available for children and teenagers younger than 20 before 2001.

The last remaining issue for the alcohol sales analysis is the decrease in excise taxes for some types of wines in 2002 close to the experiment. First, aggregated



Figure 3.2: Percent of Surveyed Men in 2004 and 1995-96 to Report Binge Drinking by Age Groups



Notes: Binge drinking for men is defined as reporting to drink at least one quarter of a bottle of spirit, or equivalently one bottle of wine or 4 cans of strong beer (5 of medium-strong beer) at one point in time at least once per month. Source: Living Conditions Report no 114, Use of alcohol and tobacco, Official Statistics of Sweden, Statistics Sweden (Statistiska centralbyrån (2007))

alcohol sales data by types of alcohol do not suggest a strong behavioural response of consumers to the decreased excise taxes on some wines, with a steady increase in wine sales already before 2002. Second, the analysis in Table 3.2, column (4) restricts the analysis to pre-July 2001 and finds a large effect of alcohol sales. This result supports the notion that overall alcohol sales are unlikely to be driven by this excise tax decrease on some types of wines. Third, I would like to emphasise that the excise tax decrease only affected certain types of wines. Regardless, if some age groups respond differently to excise tax decreases in low alcohol content wine than other age groups, the interpretation of the results might change.

### Descriptive Evidence on Crime by Age

Figure 3.3 presents average crime rates by age separated by treatment status. Averages for the main crime rate are shown in the first Figure 3.3.1. The second Figure 3.3.2 shows the corresponding average crime rates for the total crime rate. To reiterate, the main overall crime rate refers to the most severe crime within a single conviction, while the total overall crime rate refers to the total number of crimes that individuals are being convicted for. Most convictions contain more crimes than the main crime, and the total crime rate accounts for those and is about twice as large as the main

crime rate. Crime rates are computed as the number of crimes committed in each county, month and age, scaled by 100,000 male individuals. These graphs emphasize the crime age profile that is standard in the crime literature, with a sharp increase during teenage years, a peak at around 21, and decreasing thereafter. However, in my data, I observe a leveling off around the age of 30 until crime starts falling again in the mid forties.

Descriptive evidence on changes in overall crime for men before and after liberalized opening hours by ages is presented in Figure 3.4. Figure 3.4.1 shows the differences in average main crime rates after and before treatment for each age; Figure 3.4.2 presents the crime rate differences for total number of crimes. These Figures point towards increases in main crime and total crime rates for men in their twenties (20-28) and mid- to late-forties. For teenagers one can observe a reduction in crime rates following the reform. The most pronounced impact on crime appears to be a reduction in the middle ages between the ages of 30-45. Total crime rates are twice as large as main crime rates and hence the pattern in Figure 3.4.2 is more pronounced.

To better understand what is driving these changes in the main crime rates I disaggregate crime rates by seven crime categories (violent, property, fraud, drugs, traffic, others, and sex crimes). Figure 3.5 shows the changes in main crime rates before and after treatment for the corresponding types of crimes. For comparison purposes, the first Figure 3.5.1 repeats the overall changes in main crime and the following sub-Figures split up the changes in main crime by type.

For younger ages, late teens up until early thirties (ages 17-33) drug offences increase substantially relative to before the alcohol policy was implemented, with the largest relative increase for drug offences depicted for men in their early twenties (Figure 3.5.5). In addition, traffic crimes also increase for early ages 15-25 relative to the pre-policy period (Figure 3.5.6). At the same time for young men between ages 15-22 there seems to be a large reduction in property crimes, and a smaller reduction in the categories fraud and other crimes. The pattern for violent and sex crimes for early ages is not conclusive. Overall, for young ages, the descriptive evidence shows increases in drug and traffic crimes after the reform and this increase seems to be partly offset by large reductions in property crimes and somewhat by fraud and other crimes.

The reduction in crime for middle aged men (ages 30-45) shown in Figure 3.5.1 appears to be mainly driven by the crime category others for which the crime rate is

substantially lower than before the reform (between the ages 29-46), with a substantial impact also from a reduction in property crimes (for ages 28-43), which is strongest for men in their mid-thirties. Traffic crimes reduced between ages 30-40 (mainly for ages 34-41) after the reform which also contributes to the overall reduction. Fraud and tax crimes (between ages 28-42) and to a lesser degree violent crimes (between ages 32-40) also decreased for middle aged men.

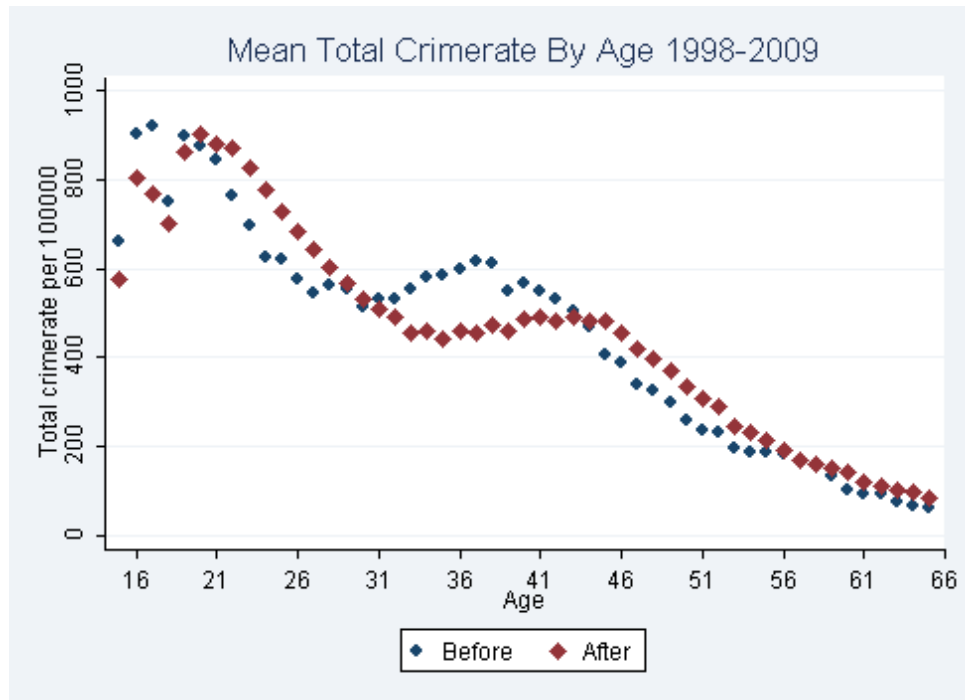
At older ages 45 through 55 traffic crimes increased relative to before the policy. This pattern is also prevalent in the drugs crime category, but weaker. It seems that traffic and drugs crimes increase later in life which makes up for the small increase in overall crime for this age group, while the overall increase in crime seems to be dampened by a reduction in other crimes for that age group.

Figure 3.3: Average Crime Rates by Age

3.3.1: Main Crime Rates



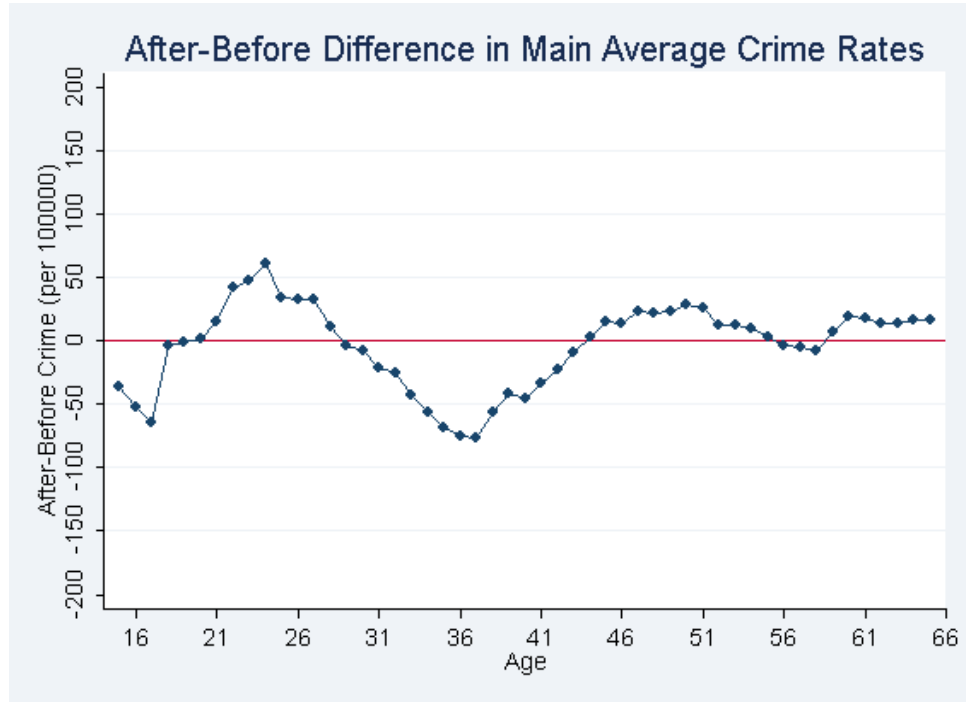
3.3.2: Total Crime Rates



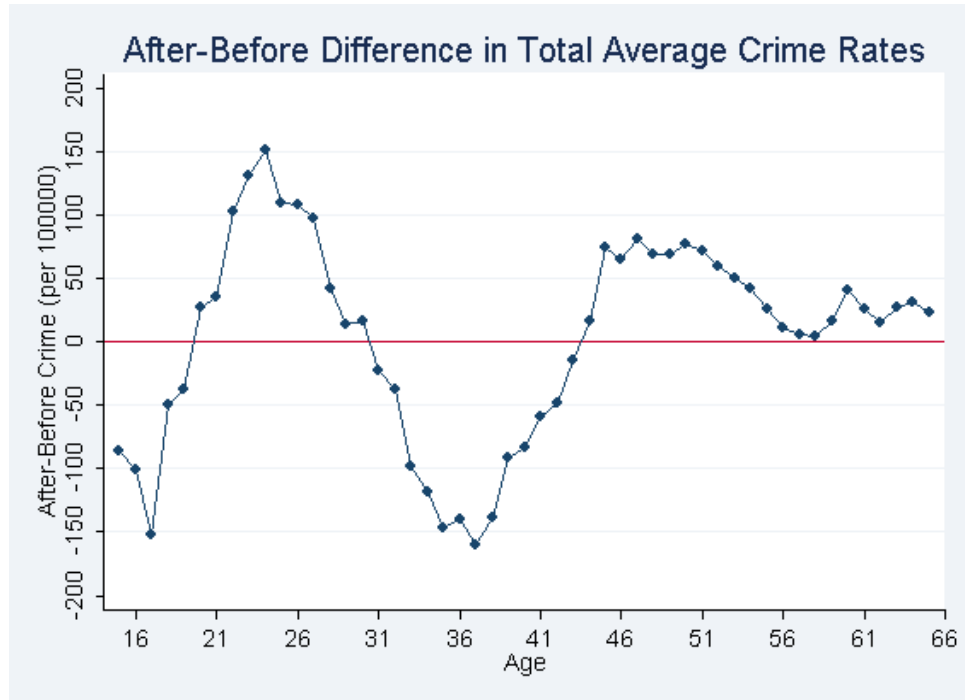
Notes: Figure 3.3.1 plots the average crime rates before (blue dots) and after (red diamonds) the reform for the main crime rate. The main crime rate is defined as the number of crimes committed in a county-month-age per 100,000 of the male population in that cell, that subsequently led to a conviction and are the main/most severe violations within a single conviction. Figure 3.3.2 shows the average crime rates before (blue dots) and after (red diamonds) the reform for the total crime rate. The total crime rate is defined as the total number of crimes committed in a county-month-age per 100,000 of the male population in that cell, that subsequently led to a conviction.

Figure 3.4: Average Crime Rates Differences by Age

3.4.1: Main Crime Rate Difference



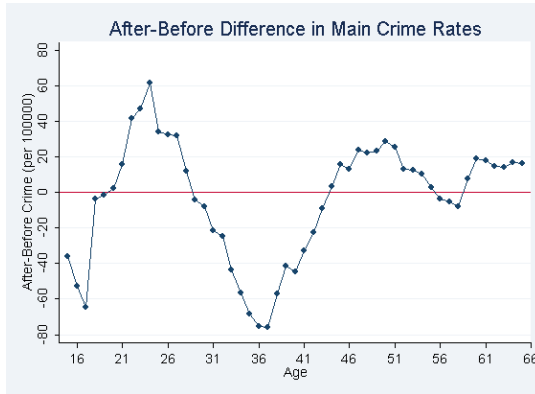
3.4.2: Total Crime Rate Difference



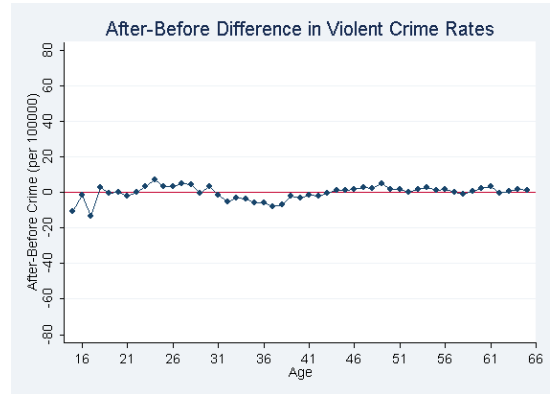
Notes: Figure 3.4.1 shows the difference in average crime rates before and after the reform for the main crime rate. The main crime rate is defined as the number of crimes committed in a county-month-age per 100,000 of the male population in that cell, that subsequently led to a conviction and are the main/most severe violations within a single conviction. Figure 3.4.2 shows the difference in average crime rates before and after the reform for the total crime rate. The total crime rate is defined as the total number of crimes committed in a county-month-age per 100,000 of the male population in that cell, that subsequently led to a conviction.

Figure 3.5: Crime Rates Differences by Types of Crimes and Age

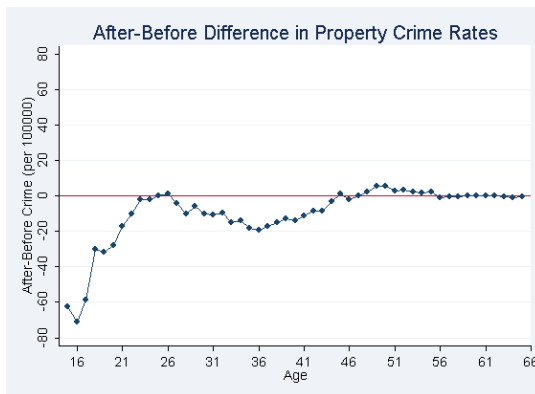
3.5.1: Main Crime Rate Difference



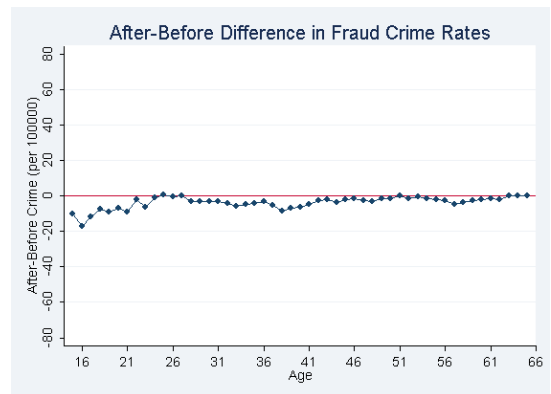
3.5.2: Violent Crime Rate Difference



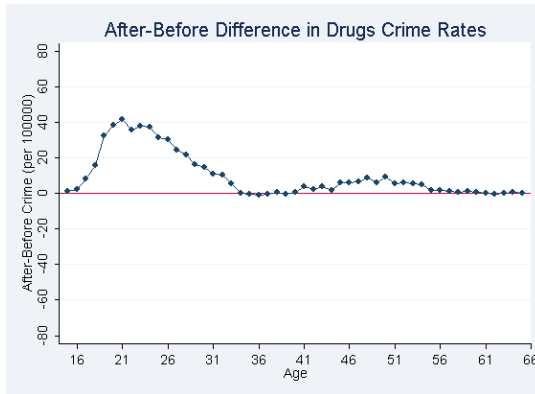
3.5.3: Property Crime Rate Difference



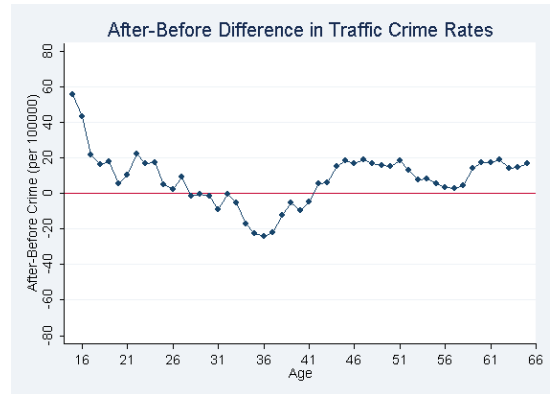
3.5.4: Fraud Crime Rate Difference



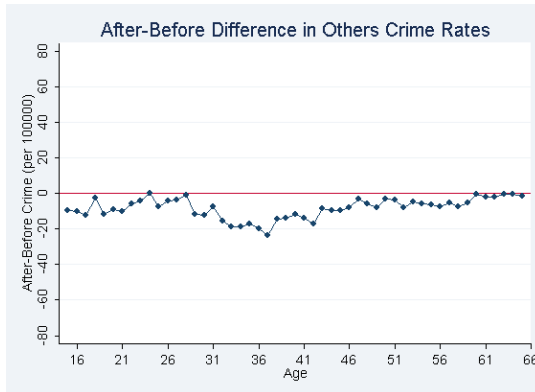
3.5.5: Drugs Crime Rate Difference



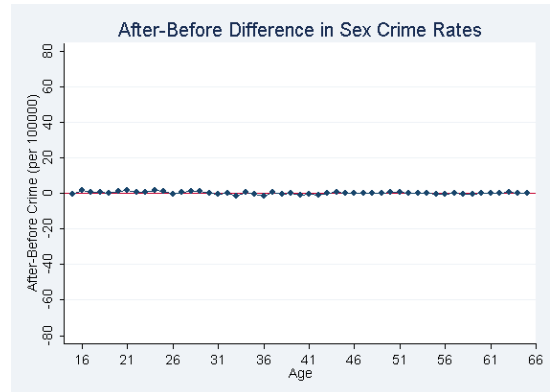
3.5.6: Traffic Crime Rate Difference



3.5.7: Others Crime Rate Difference



3.5.8: Sex Crime Rate Difference



Notes: Figure 3.5.1 shows the difference in average main crime rates after and before the reform. Figures 3.5.2-3.5.8 show the corresponding differences in average crime rates for types of the main crime.

## 3.5 Results

### 3.5.1 Alcohol Availability and Crime

The age-specific policy effects on aggregated main crime using Equation 3.2 are presented in Figure 3.6. The x-axis represents ages  $a = 16, \dots, 64$  and the y-axis shows the point estimate for  $\beta_a$  including the 95 percent confidence interval, represented by the red vertical lines. I focus on the most serious crime within a conviction, the main aggregated crime rate, for which I also have detailed information on the type of crime.<sup>17</sup>

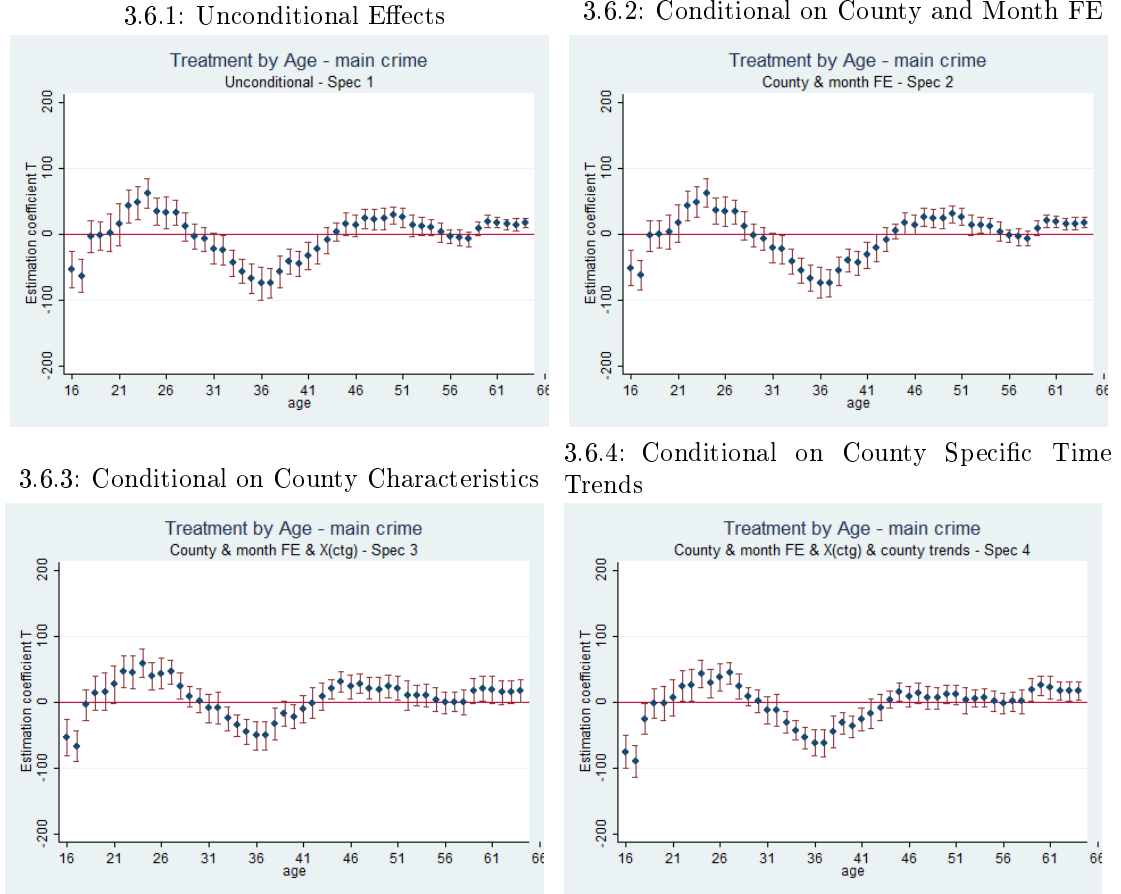
In line with the descriptive evidence in Figure 3.4.1 two age groups experience a significant reduction in total main crime following the policy: teenagers (16-17) and middle aged men (32-42). As in the descriptive evidence the magnitude of the effects is largest for men in their mid to late thirties (35-38) and for teenage boys. This pattern observed in the unconditional difference-in-difference approach shown in Figure 3.6.1 remains robust towards specifications including county and month fixed effects, county specific labour market and education variables, and county specific time trends presented in Figures 3.6.2 through 3.6.4. The effects on some ages become weaker, namely for men in their thirties and early forties. The increase in crime for men in their twenties, observed previously, remains strong and significant for men between the ages 22-28 and appears to be robust towards the inclusion of the full set of controls, fixed effects and trends presented in Equation 3.2. The positive effect later in life through the ages 45 to 55 documented by the descriptive evidence (if only weakly) remains a small effect, significant at the 5 percent level only for some ages (47-51, 44-51 including county specific controls, or only for ages 50-52 when including county specific trends) and is not robust to different specifications. Based on pre-reform crime rate levels, the magnitude of the results for teenagers corresponds to a reduction in the overall main crime rate of about 15-20 percent. For the second group that experienced a reduction in crime, men aged 32-42, the magnitude corresponds to a 9 percent reduction in crime rate. Whereas, the increase for men aged 22-28 corresponds to an increase in crime of about 10 percent based on pre-reform levels.

My results for men in their twenties relates to findings of Carpenter and Dobkin (2010) who used the variation in minimum legal drinking age across US states to argue

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<sup>17</sup>The results on total crime rates in aggregate can be found in the Robustness section in Figure 3.10.

Figure 3.6: Reform Effects on Main Crime Rate by Age



Notes: Each Figure refers to a separate specification, for the dependent variable main crime rate. The main crime rate is defined as the number of crimes committed in a county-month-age per 100,000 of the male population in that cell, that subsequently led to a conviction and are the main/most severe violations within a single conviction. Figure 3.6.1 shows the unconditional reform effects on main crime rate for each age, 3.6.2 conditions on county and month fixed effects, 3.6.3 in addition includes county specific and age specific characteristics that vary over time, and 3.6.4 adds county specific quadratic time trends to the regression. Monthly crime data by county and age from 01/1998-12/2009 are included. The plotted dots correspond to the age specific policy coefficient. The red vertical lines give the 95% confidence intervals. Estimations are based on a Prais-Winsten regression, with clustered standard errors on county level.

that alcohol consumption increases by 57 percent more days and arrest rates for just 21 year-old increased by 6 percent. The finding of Carpenter and Dobkin (2010) of an increase in the overall arrest rate for those men that are legally allowed to drink, combined with the effects that they appear to indeed drink more, is similar to my findings of making alcohol access easier and increasing overall crime rates for men in their early to mid twenties by about 10 percent. However, my observed reductions for the other two age groups is harder to relate to the existing literature.

In general, this study cannot make claims about the mechanisms behind the observed facts in the data, it can merely document the facts and provide evidence for its robustness. Importantly, the following sections will attempt to provide evidence



on which types of crimes the observed reduction/increase in overall crimes can be attributed to by age. In summary, there appears to be the three stylised facts on overall main crime that the estimations confirm from the descriptive evidence: a reduction in crime for teenagers and men in their early thirties to early forties, and an increase for those in their early to late twenties.

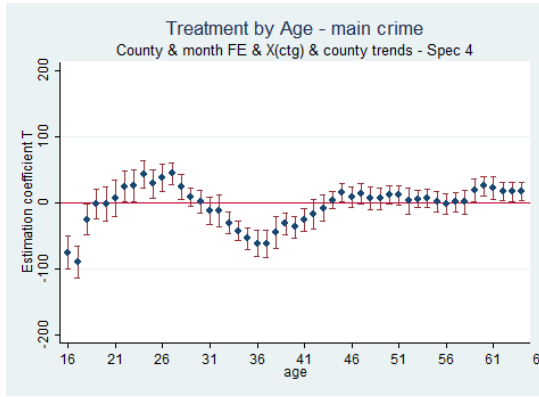
### 3.5.2 Age-Crime Profiles by Types of Crimes

To analyse in detail how the alcohol policy differentially affects types of crimes for different ages and which crimes are driving the established facts on total main crime I estimated Equation 3.2 for seven different outcome variables, corresponding to the crime rate in each county and month and age for men in each crime category  $j$ . Figure 3.7 shows the estimated age specific coefficients for each category based on the preferred specification (Spec 4), that includes county fixed effects, month fixed effects, county characteristics by age and year, and county specific quadratic trends.

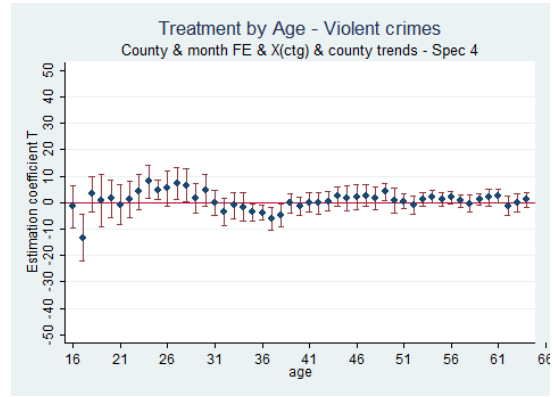
By far the largest effect by types of crimes is a large and significant increase in drug related crimes for men between the ages 19-32, presented in Figure 3.7.5. The strongest effect appears to be for the younger ones in that age group and it levels off towards the older ages substantially but remains significant. Furthermore, this increase in drug crimes is counterbalanced by a reduction in property crimes for men through the ages 18 to 22 in total. While, not significant throughout this age-group, one can also observe positive coefficients on sex crimes and violent crimes for some ages within this age group.

Figure 3.7: Reform Effects on Types of Crimes by Age

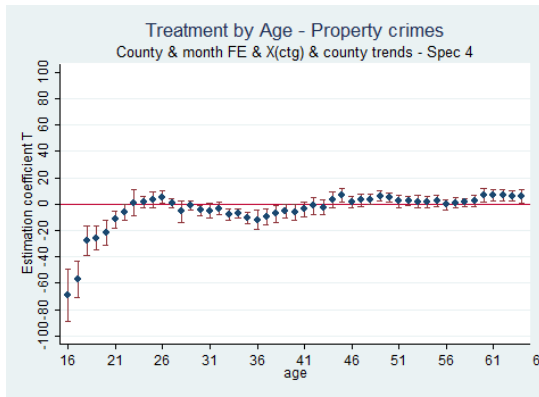
3.7.1: Total Main Crime



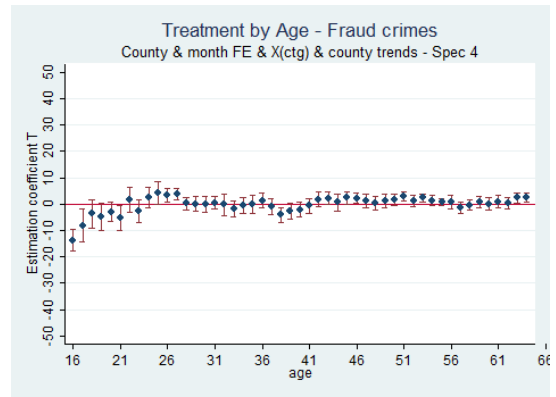
3.7.2: Violent Crime



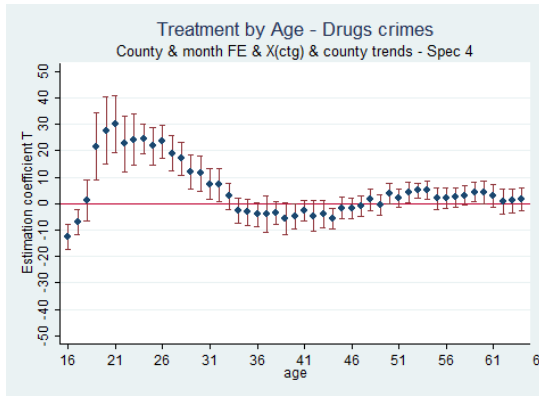
3.7.3: Property Crime



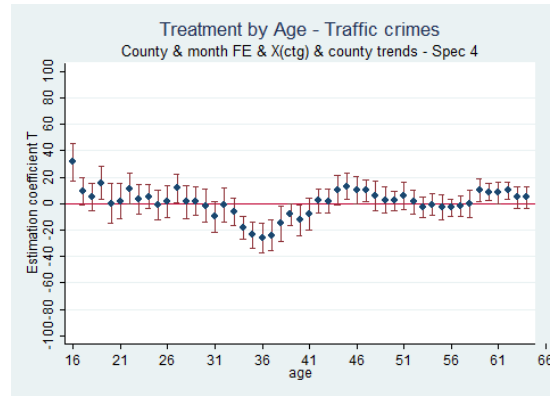
3.7.4: Fraud Crime



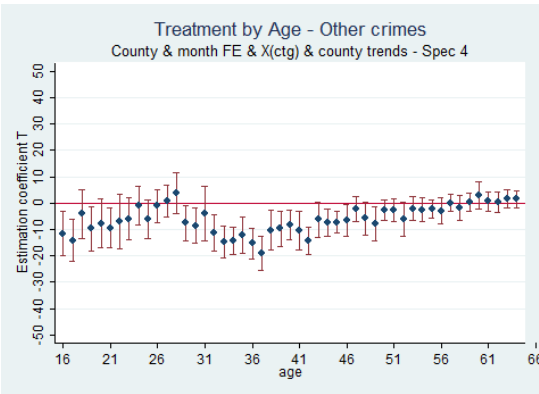
3.7.5: Drug Crime



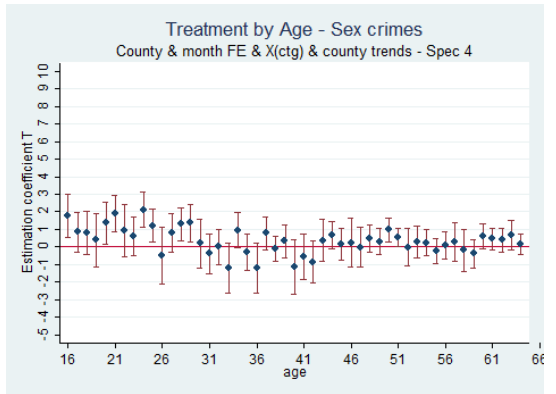
3.7.6: Traffic Crime



3.7.7: Others Crime



3.7.8: Sex Crime



Notes: Each figure shows estimation results of the same specification presented in Figure 3.6.4 for different dependent variables: crime rates of each crime type category of the main crime. Prais-Winsten regressions, clustered standard errors on county level.

Among the very young ages, there is a large and significant drop in property crimes between the ages 16-22 confirming the descriptive evidence from Figure 3.5.3. In addition, fraud, drugs and other crimes show a significant drop for teenagers too. Combining this evidence the large overall reduction in total crime for 16-17 year olds seems to stem from property, fraud, drugs and other crimes. While traffic and sex crimes are moving towards the other direction with a slight increase for teenagers.

The observed pattern of a reduction in total crime for middle aged men from the descriptives in Figure 3.4.1, that appears to be a robust and significant reduction in the estimations presented in Figure 3.6 is mainly driven by other crimes and traffic crimes. Both those crime categories experience a large and significant reduction for men in their mid thirties. The results also point towards a small significant drop in property crimes for men around 33-38, with a significant reduction for some ages in violent crimes (35-38). For drugs crimes the estimates are negative throughout this age group, though not statistically significant. The evidence on fraud and sex crimes for this age group is not robust enough to derive firm conclusion about the direction and the size of the effect.

The last pattern observed, though not robust to all specification tests, was the small increase in crime for men between 45-55. For this group the evidence in Figure 3.7.6 points towards increases in traffic crimes driving this earlier finding, but it seems to be offset somewhat by reductions in the other crimes category shown in Figure 3.7.7.

### 3.5.3 Robustness Checks

This section will further expand on robustness checks of the presented results. First, I will present three specifications that allow for more variation across ages in the preferred specification (Spec 4). More specifically, I allow linear time trends to vary by age (Specification 5) as the concern might be that different ages have different time trends in crime. Along this line, one might expect crime rates to vary across months within a year differently for different ages, and hence, I allow month fixed effects to vary by age (Specification 6). Finally, I let county specific fixed effects vary by age too. These results are shown in Figures 3.9.1-3.9.3. It turns out that both age specific month and age specific county fixed effects do not alter the results, but the inclusion of age specific linear trends leads to a substantial decrease in precision of the policy estimates.

Second, Figure 3.10 presents results on the total number of crimes. The main results presented above correspond to the most severe crime listed in a single conviction, while other crimes that are listed in the same conviction are not taken into account. The results on total number of crimes presented in Figures 3.10.1-3.11.3 show the same pattern as the main crime rate, just more pronounced and with a magnitude twice as large, which is due to the larger amount of total numbers of crimes.

The dynamics of the alcohol policy on alcohol sales showed that the impact on alcohol sales leveled off in the fourth year after the implementation of the policy (this could be due to the temporary reduction in alcohol sales in 2004-2005 following the abolishment of trade restrictions within the EU). I also estimated all the above crime specifications restricting the observation period to 1998-2003. The effects are not larger for the shorter time window, indeed the results are slightly weaker. This emphasizes that the crime rates did not only change in the short run, but pertained at the new levels even 8-9 years after the alcohol policy was introduced.

I further expanded the robustness analysis, where I split the sample into five-year age-groups and then estimate Equation 3.2. The results remain literally identical and are hence not presented here.

## 3.6 Discussion

I evaluate the impact of a Swedish alcohol policy that liberalized opening hours in the state monopoly on crime-age profiles by types of crimes. Evidence on how alcohol policies affect individuals to engage in criminal activity at different ages and what types of crimes are mostly affected is scarce. My analysis provides evidence on this link. It contributes towards the literature that aims to assess the impact of alcohol policies on crime that is currently debated around the world. More recently, policies have aimed to reduce consumption and thus crime by following more liberal alcohol policies, such as opening hours in Brazil and Sweden, the UK Licensing Act (2003), and hotly debated still are reductions in the minimum legal drinking age in the US with the *Amethyst Initiative*. The Swedish policy that this study focuses on was not aimed directly at reducing crime. The effects of this alcohol policy using individual crime register data on men between ages 17-23 has been studied earlier by Grönqvist and Niknami (2011). However, my discussion of the age-specific reform effects for all ages and types of crimes is a novel contribution. I use a panel data set on crime

by age, and types of crimes for each county and month over the period 1998-2009 to estimate the effect of the staggered implementation of liberalized opening hours of Sweden's alcohol stores that occurred during 2000-2001. The data was constructed from individual register data covering all individual convictions in Sweden with details on types of crimes. The findings of my empirical analysis show that the policy had heterogenous effects by age. Overall crime rates increased for men between the ages 20-28, and reduced for two other age groups, the very young (16-17 year olds) and middle aged men in their thirties. These findings are robust across various specifications. Splitting up the analysis by types of crimes reveals, that alcohol seems to influence different types of crimes for different age groups. At early ages property, drug and fraud crimes appear to reduce significantly which drives the overall results, while traffic crimes increase for this age group and counterbalances the overall reduction somewhat. The increase for young men in their twenties appears to be largely driven by a strong increase in drug related offences. This age group shows increases in violent and sex crimes too, but the effects are not strong throughout this age range, and not strongly significant. The last robust pattern found for middle aged men, a reduction in overall crime, is driven by a reduction in other crimes and traffic crimes.

The findings of this study must be interpreted carefully, as they cannot contribute towards explaining the mechanisms behind the observed pattern. The strongest effect of the alcohol policy on specific types of crimes is on drug related offences. This finding relates to the literature on the complementarity/substitutability of alcohol and drugs (see for example, Yörük and Yörük (2011), Crost and Guerrero (2012) Anderson and Hansen (forthcoming)). I find a heterogenous link between alcohol and drugs by age: evidence that could be interpreted as a complementarity between alcohol and drugs for men in their twenties, and substitutability for teenagers and middle aged men. However, in this study I cannot determine alcohol consumption changes of different age groups in response to the reform and hence the interpretation towards complementarity and substitutability of alcohol and drugs remains speculative. Though, my finding emphasizes differences across age groups and might help explain why different studies find different results on the substitutability versus complementarity of alcohol and drugs, as it crucially depends on the age group considered, as well as the type of policy.

My findings point weakly towards increased traffic offences of men between 16-20 when alcohol is more readily available which is in line with the age-shift in DUI

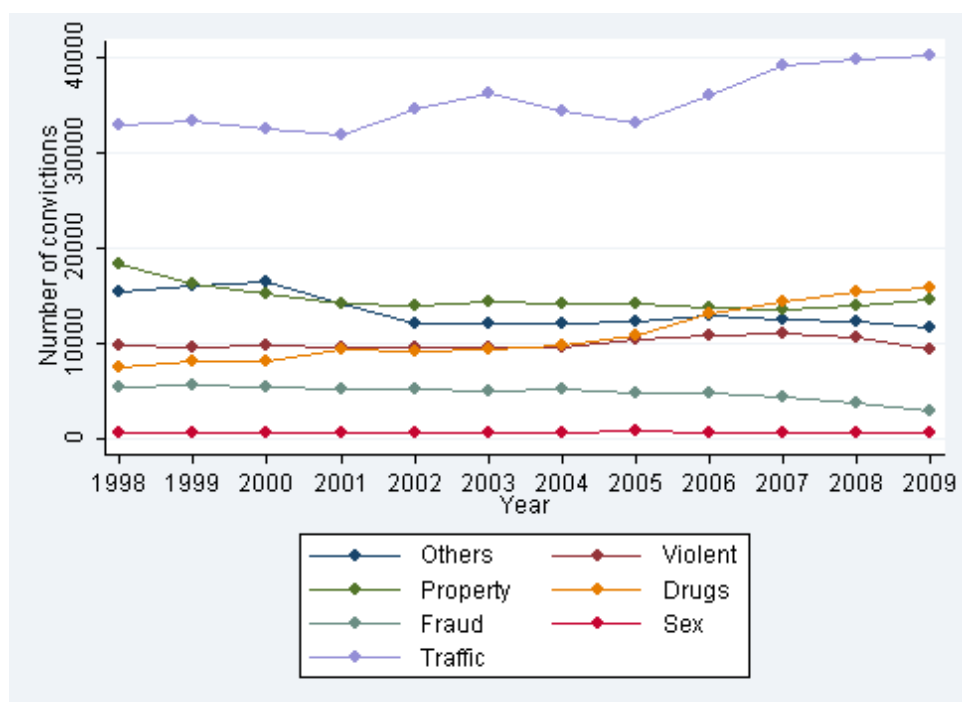
arrests found in earlier studies (see Carpenter (2005) and Carpenter (2007)). At the same time, my finding of middle aged men being less likely to commit traffic violations does not fit in what other studies have found. Anecdotal evidence suggests that police surveillance for traffic crimes was increased after the liberalization of opening hours. Norström and Skog (2005) also argues that intensified screening for drunk driving during the experimental period on Saturdays might be the possible explanation for this observed reduction in traffic violations. My descriptive evidence suggests that men in their thirties were the group most likely to be convicted for traffic crimes before the reform. More police screening after the reform could have led to a deterrence effect for the group most likely to commit traffic crimes, which could potentially explain the second robust finding of this study - the reduction in traffic crimes for middle-aged men. However, this study cannot disentangle potential mechanisms behind the observed pattern in the data and cannot determine whether a deterrence effect was at play. To be able to analyze a potential deterrence effect through more police screening additional data on police surveillance, for example the number of police officers on the streets before and after the reform would be required.

Another very speculative channel, could be that increased alcohol consumption at home reduces consumption in bars and nightclubs via a substitution effect. Alcohol bought in stores is mostly consumed at home, so increasing alcohol access in stores on the weekends might increase alcohol consumption at home and reduce consumption outside in bars and nightclubs. This might have led middle aged men to be less likely to drink and drive (because they are rather staying at home when drinking). For teenagers increasing alcohol access (of siblings and friends) might also induce more consumption at home where vandalism, violent crimes and other public disturbances are less likely to happen than in bars or nightclubs. These arguments could help explain the reduction in property crimes, violent crimes and drugs crimes, but they are mere speculation and require a more thorough analysis, based on additional data on where people consume alcohol after the reform happened.

In conclusion, this chapter provides new facts on how liberalized opening hours influence crime by age. It emphasises that alcohol liberalization can both increase and reduce crime, and that the effect of such policies are heterogenous across age groups and types of crimes. Alcohol policies can have implications for various types of crimes, in particular the link between alcohol and drugs are important. The mechanisms behind the observed pattern remain an open question for future research.

## 3.7 Appendix - Chapter 3

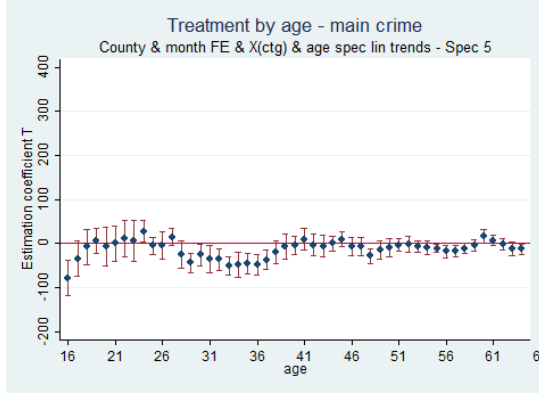
Figure 3.8: Yearly Number of Convictions by Type



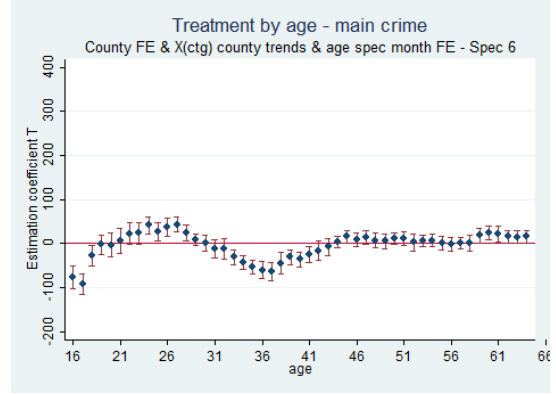
Notes: Figure shows yearly number of convictions by types of crimes.

Figure 3.9: Robustness Checks on Reform Effects on Main Crime Rates by Age

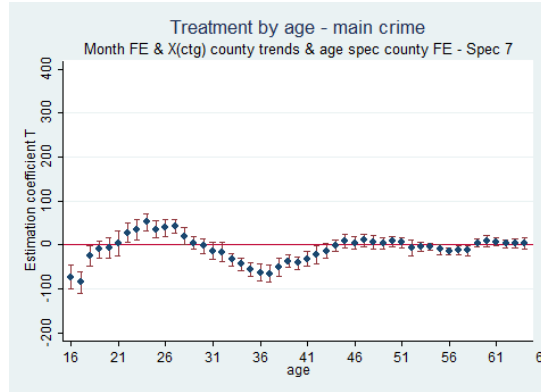
3.9.1: Age Specific Linear Time Trends



3.9.2: Age Specific Month Fixed Effects



3.9.3: Age Specific County Fixed Effects

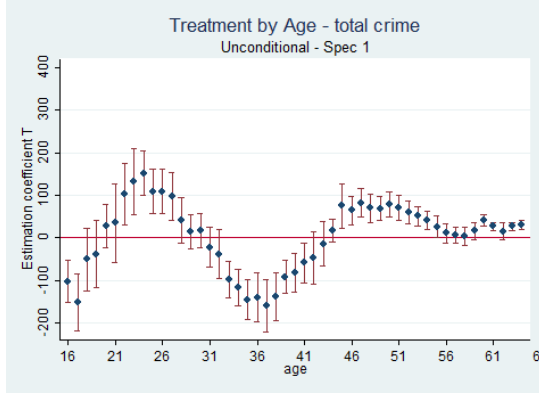


Notes: Each figure refers to a separate robustness specification, for the dependent variable main crime rate. The main crime rate is defined as the number of crimes committed in a county-month-age per 100,000 of the male population in that cell, that subsequently led to a conviction and are the main/most severe violations within a single conviction. Figure 3.9.1 includes county and month fixed effects, county characteristics and age specific linear time trends, Figure 3.9.2 includes county fixed effects, county characteristics, county specific quadratic trends and age specific month fixed effects, Figure 3.9.3 includes month fixed effects, county characteristics, county specific quadratic trends and age specific county fixed effects. Monthly crime data by county and age from 01/1998-12/2009 are included. The plotted dots correspond to the age specific policy coefficient. The red vertical lines give the 95% confidence intervals. Estimations are based on a Prais-Winsten regression, with clustered standard errors on county level.

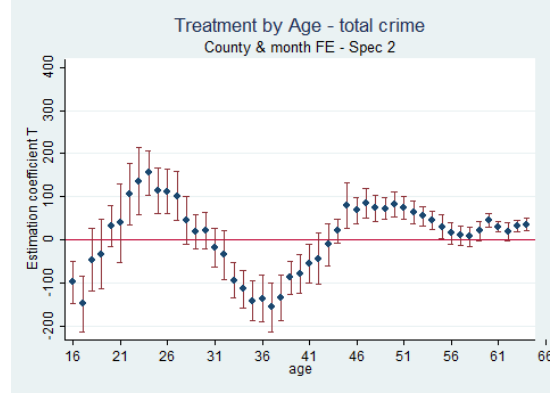


Figure 3.10: Reform Effects on Total Crime Rates by Age

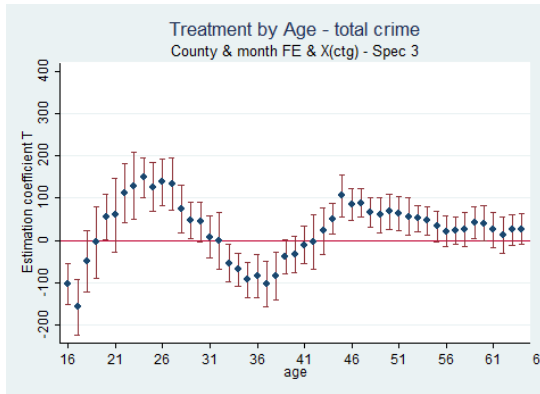
3.10.1: Unconditional Effects



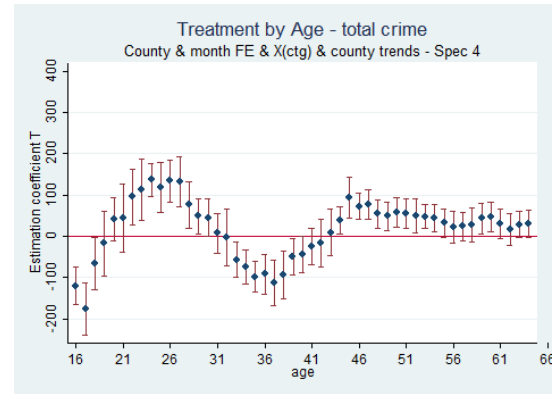
3.10.2: Conditional on County and Month FE



3.10.3: Conditional on County Characteristics



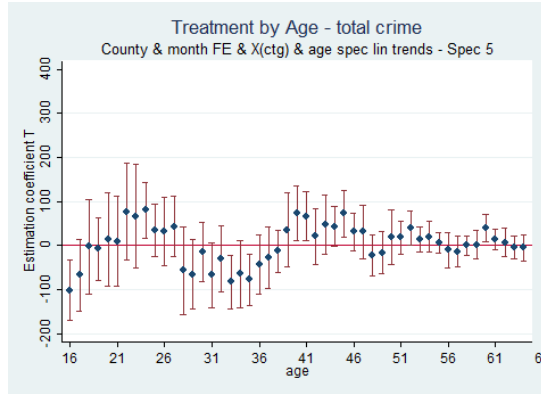
3.10.4: Conditional on County Specific Time Trends



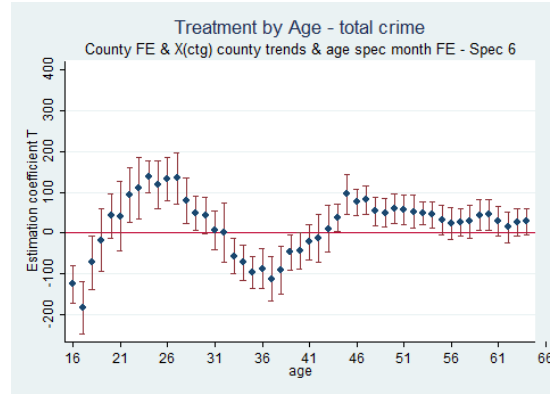
Notes: Each figure refers to a separate specification, for the dependent variable total crime rate. The total crime rate is defined as the total number of crimes committed in a county-month-age per 100,000 of the male population in that cell, that subsequently led to a conviction. Figure 3.6.1 shows the unconditional reform effects on total crime rate for each age, 3.6.2 conditions on county and month fixed effects, 3.6.3 in addition includes county specific and age specific characteristics that vary over time, and 3.6.4 adds county specific quadratic time trends to the regression. Monthly crime data by county and age from 01/1998-12/2009 are included. The plotted dots correspond to the age specific policy coefficient. The red vertical lines give the 95% confidence intervals. Estimations are based on a Prais-Winsten regression, with clustered standard errors on county level.

Figure 3.11: Robustness Checks on Reform Effects on Total Crime Rates by Age

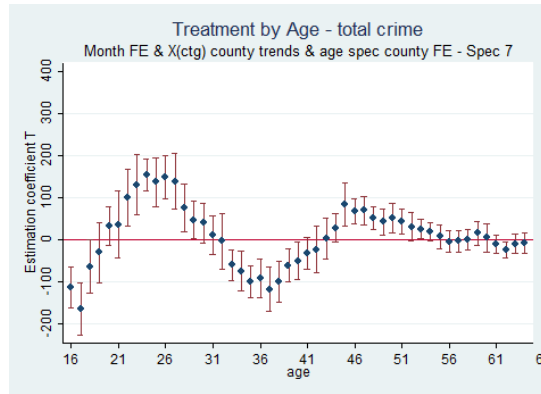
3.11.1: Age Specific Linear Time Trends



3.11.2: Age Specific Month Fixed Effects



3.11.3: Age Specific County Fixed Effects



Notes: Each figure refers to a separate robustness specification, for the dependent variable total crime rate. The total crime rate is defined as the total number of crimes committed in a county-month-age per 100,000 of the male population in that cell, that subsequently led to a conviction. Figure 3.11.1 includes county and month fixed effects, county characteristics and age specific linear time trends, Figure 3.11.2 includes county fixed effects, county characteristics, county specific quadratic trends and age specific month fixed effects, Figure 3.11.3 includes month fixed effects, county characteristics, county specific quadratic trends and age specific county fixed effects. Monthly crime data by county and age from 01/1998-12/2009 are included. The plotted dots correspond to the age specific policy coefficient. The red vertical lines give the 95% confidence intervals. Estimations are based on a Prais-Winsten regression, with clustered standard errors on county level.

## Concluding Remark

In conclusion, the main contributions of this thesis are threefold. The first main contribution of this thesis lies in confirming earlier studies on education policies and crime on the target generation. We confirm that a Swedish educational reform of the 1950s which increased compulsory schooling and abolished tracking substantially reduced crime of men who were directly affected by the reform. Our data only enables us to start observing them from the age of 18-21 onwards, which means we are not able to map out a complete picture from the beginning of their criminal career. Despite this limitation, for the selected cohorts, we are able to observe them for an age with highest criminal activity. We document a reduction of overall crime by 1.5 percentage points, with the effects of the reform being strongest for ages 20-24. The reduction in overall crime rates are shown to be mainly driven by a reduction in property crimes and serious traffic crimes that led to a guilty court verdict. Violent and drug-related crimes remained unaffected. Furthermore, the reform also reduced repeated offences for men in the relevant cohorts.

The overall finding of reduced crime rates following an increase in compulsory schooling laws is consistent with results found in earlier studies for the US in Lochner and Moretti (2004), and for the UK in Machin, Marie, and Vujić (2011). The contribution to the existing literature that this thesis provides is to confirm previous results of a substantial crime reduction following the increase in compulsory schooling for Sweden. More importantly, this study employs more detailed individual level register data on crime allowing us to link individuals to convictions. The data also enables us to make statements about education policy impacts on different types of crimes and at different ages of the life cycle. The findings are important because they highlight the broader impact of education policies, and a way to improve other outcomes than education as summarized in the context of education and crime in Lochner (2011). Overall, these findings confirm the previous studies in this field, and support the idea that there are ways to reduce crime that go beyond deterrence and punishment, while at the same time having other positive impacts, i.e. increasing education and labour market opportunities.

Future research in this area is limited by the mentioned existing studies relating to this question with the first papers by Lochner and Moretti (2004), and Machin, Marie, and Vujić (2011)), our study, and another Swedish study by Hjalmarsson,

Holmlund, and Lindquist (2011) that focuses on the first generation impact on crime only. The literature has established that there exist secondary effects of education policy on own crime. Remaining interesting directions for future research would be an analysis of the mechanisms behind reduced crime for the target generation. It remains an open question through which channel increased education reduces crime. Insights into understanding whether it is improved economic opportunities or social values and social skills that prevents individuals to engage in criminal activity would be of high importance to the literature on the economics of crime.

The second and more important contribution of this thesis in relation to the literature of education policy and crime is to show that the Swedish education reform had an intergenerational effect on crime. This is a novel contribution to the literature and addresses a new question whether exposing fathers to the reform has an impact on the crime rates of the next generation. Our results establish substantial impacts of father's exposure to the reform on crime rates of sons in the child generation: it resulted in an overall decline in the crime rate by about 0.8 percentage points, mostly driven by a decline in convictions among the 15-19 year olds. The reductions are mainly concentrated among violent crime, traffic crime and fraud.

This finding of decreased child crime following an education reform has a large significance in the literature. It highlights that education policies can help reduce crime not only for the target generation but also across generations. It can be placed within the literature that aims at evaluating educational reforms and their impacts on educational attainments of the next generation, see Black, Devereux, and Salvanes (2005), and Holmlund, Lindhal, and Plug (2011). Furthermore, previous literature suggests that we might expect improved parental human capital to translate to better parenting and greater availability of resources, which may thereby improve child crime outcomes. Our data allows us to establish that home environments for children in families where the father was exposed to the reform improved in a number of dimensions (earnings, education etc.) through which it might lead to a reduction in criminality of their children. These findings are consistent with both theories of intergenerational transmission of human capital (see e.g. Becker and Tomes, 1979) as well as sociological theories on the effect of strains (see Merton (1938)) and formation of social capital (see Coleman (1988)). It also relates to direct evidence on how improved childhood environments and early education can reduce crime as shown in evidence on the Perry pre-school experiment by Schweinhart, Montie, Xiang, Barnett,

Belfield, and Nores (2005), and Cunha and Heckman (2007).

What we cannot learn from our research design, however, are what mechanisms are driving these impacts intergenerationally. With the information available we are not able to conclusively establish the mechanisms that led to such a reduction in sons' crime. Future research could be directed towards helping understand those links better and try to determine the mechanisms behind a reduction in child crime following education policies that affected their fathers. Another important direction of future research could be the magnitude of the effect. The size and importance of the intergenerational effect relative to improving child outcomes directly remains an open question. Finally, the finding of mother's reform assignment having no impact on son's crime could be examined in more detail as well, since previous literature has highlighted the importance of mother's education on child outcomes, for example Carneiro, Meghir, and Parey (2013).

The third main contribution of this thesis is to show evidence of how a Swedish alcohol policy affects individuals to engage in criminal activity at different ages and what types of crimes are mostly affected. The focus of the third chapter shifts towards the link between alcohol and crime and thereby focuses on the situational context in criminal activity. General statistics from the US and UK highlight the correlation between alcohol and crime: 33 percent of state prisoners in the US reported to have been under the influence of alcohol at the time of the offence in 2004 (Bureau of Justice Statistics (2004)); in the UK 47 percent of victims of violent incidents perceived their offender(s) to be under the influence of alcohol (Crime Survey for England and Wales (2013)). As highlighted in detail in chapter three there exists a large literature aiming to determine the link between alcohol and crime and different types of alcohol policies and their impacts on crime, for example related literature on opening hours of bars and alcohol stores in Brazil and Sweden (see Biderman, De Mello, and Schneider (2010), Grönqvist and Niknami (2011), Norström and Skog (2005)) and various studies using laws on the minimum legal drinking age in the US (see Carpenter and Dobkin (2011), Carpenter (2007)).

The specific alcohol policy evaluated here is the Swedish alcohol policy that liberalized opening hours in the state monopoly during the years 2000-2001. The staggered implementation of liberalized opening hours of Sweden's alcohol stores is used to analyse this link using a panel data set on crime by age, and types of crimes for each county and month over the period 1998-2009. The main findings of the empirical analysis

emphasize that the policy had heterogeneous effects by age. In contrast, the earlier study by Grönqvist and Niknami (2011) that uses the same alcohol policy and the same individual crime register data has only focused on men between ages 17-23. This thesis shows that overall crime rates for men aged 20-28 increased, while crime rates for very young men aged 16-17, and middle-aged men in their thirties reduced. Splitting up the analysis by types of crimes reveals that drug and fraud crimes appear to reduce significantly during early ages, which drives the overall results and at the same time traffic crimes increase and counterbalances the overall reduction. For men in their early twenties the majority of the increase in overall crimes can be attributed to a strong increase in drug related offences. Other crimes and especially traffic crimes, appear to be driving the reduction in overall crime rates for middle aged men. Perhaps surprisingly, violent and sex crimes cannot be shown to be influenced by this alcohol policy.

The type of crime most affected by the change in the alcohol policy are drug related offences, for which the third chapter documents a heterogeneous link between alcohol and drugs by age. Increased drug offences for men in their twenties, and decreased drug offences for teenagers emphasizes differences across age groups. Different studies find different results on the substitutability versus complementarity of alcohol and drugs (see Yörük and Yörük (2011), Crost and Guerrero (2012), and Anderson and Hansen (forthcoming)) which appears to depend on the type of policy, type of crime and age group considered. The attempt to relate the findings of chapter three to the literature on the complementarity/substitutability of alcohol and drugs is limited by the fact that I cannot determine changes in alcohol consumption of different age groups in response to the reform. This means the interpretation towards complementarity and substitutability of alcohol and drugs remains speculative. Future research in this area should aim to establish exactly which ages will see a change to their alcohol consumption following alcohol policies. Based on this one could then map towards alcohol related crime engagement.

By the nature of the offence, driving under the influence of alcohol is often considered when alcohol policies are evaluated towards their impact on crime. The findings in the third chapter point weakly towards increased traffic offences for men aged 16-20 when alcohol is more readily available which is in line with the age-shift in DUI arrests found in earlier studies (see Carpenter (2005) and Carpenter (2007)). At the same time, the finding of middle aged men appearing to be less likely to commit

traffic violations contrasts the findings of other studies. Anecdotal evidence for the Swedish alcohol reform exploited in this study, suggests that police surveillance for traffic crimes was increased after the liberalization of opening hours. Norström and Skog (2005) also argues that intensified screening for drunk driving during the experimental period on Saturdays might be the possible explanation for this observed reduction in traffic violations. More police screening after the reform could have led to a deterrence effect for the group most likely to commit traffic crimes (men in their thirties), which could potentially explain the second robust finding of this study - the reduction in traffic crimes for middle-aged men. Regarding those traffic offences, however, this study is limited by not being able to determine whether a deterrence effect was at play. To determine whether a deterrence effect was at play one would require additional data on police surveillance, for example the number of police officers on the streets before and after the reform.

Finally, when considering the link between crime and alcohol consumption it is important to what degree there exist social multiplier effects. Alcohol consumption in bars and nightclubs could potentially increase the risk of vandalism, violent crimes and other public disturbances behaviour relative to alcohol consumption at home. We might expect that alcohol consumption in bars and nightclubs is reduced via a substitution effect due to increased alcohol consumption at home (triggered by increased access to alcohol in stores). Importantly, we might expect that to have interaction effects with crime. While these arguments could help explain the observed crime patterns, they remain mere speculation and require a more thorough analysis. Future research should be concerned with additional data on where people consume alcohol after the reform happened. This puts high demands on the data, which was not available in this study. Overall, the major limitation of the third chapter that one cannot disentangle potential mechanisms behind the observed pattern with the existing data points towards remaining open questions for future research in this area.

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